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Wm Oulston Professor

PHILOSOPHICAL
ESSAYS.

BY

HENRY EELES, Esq;

CONTAINING

His Diſcovery of the Cauſe of THUNDER, with a ſubſequent Explanation and Demonſtration of the ſame.

The Cauſe of the Aſcent, Station, and Deſcent of VAPOURS and EXHALATIONS. The Cauſe of WINDS, and an Explanation of the general Phænomena of the WEATHER, BAROMETER, &c.

The true Rudiments and Theory of ELECTRICITY and MAGNETISM.

An Attempt to ſhew that the ELECTRICAL POWERS are the Cauſe of the REFLECTION and REFRACTION of LIGHT.

Some Mention of the wonderful MEDICINAL EFFECTS of ELECTRICITY on a great Number of Patients, whom he has cured of Palſies, and many other Diſorders. With other uſeful Matters, which may be deduced from theſe ESSAYS.

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PREFACE

TO THE

LETTERS.

NEWTON in his immortal reſearches diſcovered, by effects which were ſenſible to him, that ſome ſubtile medium, of great elaſtic force, muſt neceſſarily ſupervade the ſurfaces of all bodies, to cauſe the reflection and refraction of light, &c. but being profeſſedly ignorant of what that medium or power may be, he ſaid that he would call it æther.

From the modern diſcoveries about electricity, many gentlemen have declared that electricity was the æther of Sir *Iſaac Newton*; but being ignorant of the mediums or powers which conſtitute what we call electricity, and conſequently ignorant of the nature and properties or general laws by which thoſe powers act, it was impoſſible for them to ſhew how electricity was adapted to the purpoſes, to which that great man applied his æther.

For hitherto electricity (except what may be found in the following tracts, and what I ſhall mention) has been conſidered as a ſubtile medium ſubſiſting they knew not where, and acting they knew not how. I now find that ſome gentlemen, who firſt enquired into this matter, ſuppoſed that there were two kinds of electricity; and ſome others followed their opinion; particularly Mr. *Muſchenbrock* has aſſerted that there were two kinds of electricity, and that any body may be endued with either kind at pleaſure; but that it was impoſſible that any body ſhould be in poſſeſſion of both at once; but where that gentleman has been miſtaken, the following tracts will ſhew; and that it was impoſſible for him to have explained, his own famed and very uſeful experiment of charging a bottle, by that doctrine.

Mr. *Benjamin Franklin*, who has been greatly followed, eſpecially by the Engliſh, has produced another doctrine of electricity. He ſuppoſes that electricity ſubſiſts in glaſs, and that glaſs by being rubbed, throws out that power, and that ſealing-wax, reſins, &c. drink it in. And in electrifying other bodies, he ſays that ſome may be electrified poſitively and ſome negatively; which are terms I muſt confeſs that I do not underſtand; but what I think he muſt mean by them is, that when a body is poſitively electrified, there muſt be part of that power which is thrown out from the glaſs, ſtopped or retained in that body; and that muſt be the cauſe of that body's repelling another body ſo electrified; and when a body is negatively electrified, it muſt be diveſted of part of its natural ſhare of electricity; but if electricity ſubſiſts only in glaſs, and not in all other bodies, how is it poſſible to diveſt a body of what it never had? and he ſays that two bodies negatively electrified, repel each other as much as when poſitively electrified. But to me it appears a perfect contradiction to ſay that two bodies ſhall repel each other the farther, the more they are diveſted of the repulſive power. This plain contradiction, one would think, might have made this ingenious gentleman ſuſpect that there was ſomething fallible or deficient in his doctrine of poſitive and negative electricity; yet in ſome experiments communicated to the Royal Society, he makes it a principle in his doctrine of electricity; where he has mentioned three under the expreſs title of principles, in the 49th Volume of Philoſ. Tranſ. page 300.

His firſt principle. Electric atmoſpheres that flow round non-electric bodies, being brought near each other, do not readily mix and unite into one atmoſphere, but remain ſeparate, and repel each other.

Second principle. An electric atmoſphere not only repels another electric atmoſphere, but will alſo repel the electric matter contained in the ſubſtance of a body approaching it: And without joining or mixing with it, force it to other parts of the body that contained it.

Third principle. Bodies electrified negatively, or deprived of their natural quantity of electricity, repel each other, (or at leaſt appear to do ſo, by a mutual receding) as well as thoſe electrified poſitively, or which have electric atmoſpheres.

His firſt principle is true, when the atmoſpheres are of one kind; but quite falſe when they are of different kinds; for there they attract each other and inſtantly join; as will be ſeen in the following, and all other experiments.

In his ſecond principle he allows that the electric matter exiſts in all bodies; tho' in ſeveral of his writings he confines it to glaſs. But before we can know how an electric atmoſphere acts on this electric matter, we muſt enquire what this electric matter is, and of what it conſiſts, and how it acts and is acted on. If it conſiſted of a ſingle medium or power, his doctrine would in ſome meaſure be true, but the effects which always appear would not follow. But this electric matter, or what we call electricity, conſiſts of two different diſtinct elaſtic mediums or powers, which equally and ſtrongly attract and condenſe each other, and are equally attracted by all matter, according to the ſpecific gravity of the matter, as will appear in the following tracts. Therefore when any body is immerged in an electric atmoſphere, that atmoſphere only repels that power of electricity which is of the ſame kind in the body; but equally attracts the power of a different kind which is in the body; and while that body remains ſo immerged, the different powers in that body remain ſeparate with their different atmoſpheres at each end of that body; which will be more particularly explained in what I call an experimentum Crucis, and many other experiments in the following tracts.

As to his third principle, it is quite erroneous to ſuppoſe that bodies by being negatively electrified (as he terms it) are deprived of their natural ſhare of electricity: for they are no more deprived of that, than when they are poſitively electrified; and I am ſurpriſed to find that a gentleman, ſo converſant with electricity, ſhould think that bodies electrified negatively, are without electric atmoſpheres; when all experiments ſhew that they have as extenſive atmoſpheres as when electrified poſitively; and that they act in all reſpects in the ſame manner. For when a body is electrified negatively, it is impoſſible for him or any one elſe to tell, by any effects of that body on another body which is not electrified, by which power that body is electrified; nor is there any way of diſcovering it, but by bringing another body electrified with a known power, to try whether its atmoſphere attracts or repels the atmoſphere of the body electrified with an unknown power.

Now it is plain from theſe principles, and by all the other writings of this gentleman, which I have ſeen, that he makes electricity conſiſt of one ſingle medium or power; without ſhewing how this power may be found with an extenſive atmoſphere, or how that atmoſphere can produce the phænomena which ariſe fom electrical experiments. We are told that friction makes glaſs throw it out, and that ſealing-wax, reſins, ſulphur, &c. drink it in. But how is this done? This ſuppoſition is very fallible; for I can make glaſs throw out the reſinous or vitreous power at pleaſure, or I can make ſealing-wax throw out the vitreous or reſinous power as often as I think fit, and I can alter this action of the wax twenty times in a minute. I call the powers reſinous and vitreous for diſtinction ſake; for I do not know any eſſential difference to diſtinguiſh them by.

But ſuppoſing there was but one kind of electric atmoſphere; by what action can it diveſt a body of its natural ſhare of the electric matter? ſince this gentleman allows that the electric matter exiſts in all bodies. It is intelligible how it may add to it, but not how it can take from it. In his explaining the charging and diſcharging the *Leyden* bottle, he is obliged to have recourſe to a ſtrange hypotheſis; that there is ſome ſtratum or partition in the middle of glaſs which ſeparates the two ſides; and that when the electric fire is accumulated on one ſide of the glaſs, the other ſide parts with as much of its natural ſhare, as was received by the firſt; and that when a communication is made between the ſides, by any conductor of electricity, the emptied ſide greedily drinks up the electric matter accumulated on the other ſide. But this is an entire miſtake; for when the bottle is charged, it is equally electrified on both ſides, but with the different powers of electricity; and when a communication is made by a conductor, the increaſed power without, flies in; and the increaſed power within flies out; to make the powers equal to each other within and without; for theſe powers ſtrongly attract and condenſe each other when equal to each other; which is their natural ſtate in all bodies; and therefore they do not exert any ſenſible action in that ſtate. Glaſs may be equally electrified on both ſides with either the reſinous or vitreous power; and this may be done by excited glaſs or wax when they throw out the uſual powers; or it may be done by the atmoſphere of any body electrified by glaſs or wax; or the *Leyden* bottle, ſuſpended by ſilk, may be charged with either of the powers inſide and the other power outſide, by only immerging it in the atmoſphere of a body electrified with either of the powers; and may be diſcharged, and recharged in the different order, without contact or communication with any thing but that atmoſphere. Theſe experiments, or any other, cannot be explained by the action of one ſingle medium or power. From this imaginary ſtratum or partition in glaſs, this gentleman concludes that glaſs is impervious to the electric powers; but in this he is alſo miſtaken; for with excited wax or glaſs, I can electrify a body through glaſs, with which ever power a ſpectator ſhall pleaſe to name at the inſtant of performance. I have ſhewn theſe, and many other experiments to a great number of gentlemen, ſome of them converſant with electricity, who thought them ſufficient to demonſtrate to the meaneſt capacity, that two different diſtinct mediums or powers did exiſt in electricity; and that they acted by the laws which I have mentioned in my letters. But as the deſign of theſe letters was to ſhew the exiſtence of theſe powers, and by experiments to demonſtrate the general laws by which they acted; I ſhall refer to them.

The letters, which contain the doctrine of theſe different powers in electricity, were ſent to the Royal Society, on the 9th of Auguſt, 1757, and 14th of February, 1758.

In the end of the year 1759, in the vol. Tranſactions, page 371, *Robert Symmer*, eſq; has produced a doctrine; that electricity does not conſiſt of one poſitive power, as generally ſuppoſed; but of two diſtinct, poſitive and active powers; which, by contraſting, and, as it were, counter-acting each other, produce the various phænomena of electricity. I here uſe his own expreſſions.

At firſt I really thought that this gentleman might have taken hints from my letters;

but, on more mature conſideration, I think his own experiments might have led him to this doctrine; in which, as far as he has gone, he is certainly right; but he went but a very little way to explain this matter. But, as Mr. *Symmer* ſays, that Mr. *Franklin* could not be brought to agree with him; I ſuppoſe is the reaſon that Mr. *Symmer* has not ſince purſued his diſcovery. I have tried his experiment with black and white ſilk ſtockings; and find it true, that the black ſtocking is always electrified with the ſame power, whether it is put inſide or outſide; and the white ſtocking always electrified with the contrary power. This experiment is a very extraordinary one; and, I think, may poſſibly lead to a diſcovery how the clouds come to be electrified with the different powers of electricity; which is the great deſideratum, to demonſtrate the whole actions of the clouds, in their aſcent and deſcent, and relative to thunder, rain, &c. But the want of this knowledge does not any way invalidate, or retract from what I have ſaid on this ſubject; ſince the clouds are found to be really ſo electrified.

In the year 1767, Mr. *Joſeph Prieſtley* publiſhed his hiſtory of electricity; in which he attempts to mend Mr. *Symmer*'s theory, by a ſuppoſed theory of his own. But he ſo regularly ſuppoſed the theory which I gave in the following tracts, (long before Mr. *Symmer* or he thought of it) and uſed my terms and expreſſions, that I cannot forbear thinking that he had better have brought my experiments to prove it, (ſince he produced none of his own) and put my name to it, than to have left it a ſuppoſitious theory of his own. Whether, in this, he is, or is not a plain plagiary, I muſt ſubmit to the readers of both.

It will plainly appear, that I have not ſuppoſed the exiſtence of theſe different powers; but that I have demonſtrated by the effects of electrical experiments that they do really exiſt; and have alſo thereby ſhewn the general laws by which they act, and are acted on. And, if more experiments are wanted, I could give a thouſand; nor did I ever meet with any experiment which did in the leaſt contradict what I have ſaid of theſe powers, or their manner of acting; though, for many years paſt, I have applied them to various purpoſes; particularly to medicinal uſe; by which I have cured above five hundred patients of different diſorders, without applying any other medicine internally or externally. Among which were many paralytics; ſome with hemiplegias, ſome with paraplegias, ſome with palſeys, that began in the extremities, and worked up beyond the waiſt, ſo that they knew not when their excrements went from them. I have cured rheumatiſms (not of the inflammatory kind) almoſt inſtantaneouſly, in a miraculous manner; agues, jaundice, obſtructions in women, and many other diſorders, in which I thought the effects of theſe powers might do ſervice. Several phyſicians have ſubmitted to be my patients, when the known materia medica, after long trial, has not anſwered their purpoſe. I offered to ſend an account of theſe cures, and the manner of performing, to the Royal Society; but, as they did not think proper to anſwer my letter, I did not think it worth the while to trouble myſelf with ſo prolix a detail as was neceſſary to explain this affair; and as to their not anſwering, or publiſhing many of my letters, I think that I have great reaſon to complain; as I ſhall ſhew preſently.

But, to return to my friend, Mr. *Prieſtley*, who has ſo methodically ſuppoſed my theory of electricity. Though he is ſo good at ſuppoſing, can he poſſibly ſuppoſe that any man will think that he could ſuppoſe ſo regular a theory of electricity, which ſo eaſily accounts for all the phænomena thereof, and which has puzzled all the ingenious men of the world to diſcover, without his producing one ſingle experiment to ſhew the exiſtence of the different powers, or their manner of acting; or, when any man has read theſe letters, which were addreſſed to the Royal Society, of which Mr. *Prieſtley* is a Fellow, muſt he not think that Mr. *Prieſtley* has copied his theory from them? as, indeed, he has followed them very cloſe in his ſuppoſitions; for he only ſuppoſes the actions of theſe powers all along; but he has neglected to ſuppoſe the principal action, which I have mentioned, and which removes all difficulty in explaining all their other actions, *viz.* That, when they are equal to each other, they not only attract each other, but condenſe each other into almoſt an inſenſible compaſs; which is their general ſtate in all bodies, as they are both equally attracted by all matter; and that when they are any way ſeparated, which muſt be done by encreaſing the one and leſſening the other power, for they never can be entirely ſeparated in any body, the encreaſed power by its elaſtic force will expand itſelf through a ſpace many millions of times greater than what it took up in its condenſed ſtate; as I have often proved by experiments ſubject to ſenſe.

Now, if any gentleman can think this hypotheſis is Mr. *Prieſtley's* own, he muſt, at leaſt, do me the honour to ſay, I have been ten years before him; and, if real experiments are better proofs of the truth of ſuch a doctrine, than bare ſuppoſitions, he muſt alſo allow that I have proved it better and clearer than Mr. *Prieſtley* has done. Had the Royal Society thought fit to publiſh theſe letters, it would have ſaved Mr. *Prieſtley* the trouble of ſo much ſuppoſing; but I muſt take the liberty of ſuppoſing that Mr. *Prieſtley* thought I was dead, and that theſe letters would never come to light.

Before I ſpeak of my deſign in publiſhing theſe letters, I will ſhew that the doctrine given by Mr. *Franklin* and others, is fallible and inſufficient.

Firſt, Mr. *Franklin* ſuppoſes that glaſs is impervious to the electric powers. This may be confuted by ſo eaſy experiments, that I am ſurprized that Mr. *Franklin* could have miſſed them. Take a piece of bog-down, ſuſpend it by ſilk; then take a pane of clean ſaſh-glaſs, and warm it, and let the down hang by the ſide of it; then bring an excited electric to the other ſide of the glaſs, and the down ſhall fly off perfectly electrified, in the ſame manner as it would have been if the glaſs had not intervened. If the excited electric be glaſs, the down will be electrified with the vitreous power; and if wax, with the reſinous power. This ſhews that the powers paſs entire through the glaſs; but if Mr. *Franklin* chooſes to think that they are different powers, thrown from the oppoſite ſide of the glaſs, he muſt then allow that two different diſtinct powers do exiſt in all glaſs; which overſets his whole doctrine, which depends upon the action of one ſingle power. In the next place, Mr. *Franklin* ſays that glaſs cannot receive electricity at one ſide, without parting with ſo much of its natural ſhare at the other ſide; and on this ſuppoſition depends his doctrine of charging the *Leyden* bottle. But in this he is entirely miſtaken; for you cannot electrify a pane of glaſs at one ſide, but the other ſide will be equally electrified with the ſame power, whether reſinous or vitreous; except you form a communication from one ſide of the glaſs with ſome non-electrics, while you electrify the other ſide; and then, be your excited electric either reſinous or vitreous, it will repel the power of the ſame kind from the oppoſite ſide of the glaſs, and attract the different power from the non-electrics; and theſe powers will hold each other affixed to the glaſs, by their natural attraction through the glaſs; and thus the glaſs may be electrified with the different powers at each ſide; and thus the *Leyden* bottle becomes charged; for if you ſet your bottle upon a clean electric ſtand, ſo that a different power from that you attempt to charge it with cannot be drawn up to the outſide, you'll find that both ſides are electrified with the ſame power whether vitreous or reſinous.

But, to electrify a pane of glaſs equally at both ſides, either with the reſinous or vitreous power, and that by the atmoſphere of a conductor electrified by either of the powers. Paſs ten ſtrong knitting needles through a ſtick; ſo that they may lie parallel to each other, (and if they are pointed it will do beſt) and within leſs than half an inch of each other; then fix a wax or glaſs handle to the ſtick; when your conductor is electrified, and the wheel turning, bring your pane of glaſs within three inches of the conductor, and draw the points of your wires which are next to the conductor three or four times ſlowly over the glaſs, beginning at the ſide of the glaſs next to the conductor, and drawing them from the conductor, and you ſhall find that glaſs equally electrified at both ſides, with the power contrary to that which the conductor throws out: Or, if you would have the glaſs electrified with the ſame power, which the conductor throws out, place one end of the wires near the conductor, and draw your glaſs under or over the other points of the wires from the conductor, but in contact with the wires, and the glaſs will be electrified equally at both ſides with the ſame power with the conductor. Or a pane of glaſs may be electrified equally at both ſides with the vitreous power, by a large ſtick of ſealing-wax excited; or equally at both ſides with the reſinous power, by an excited glaſs tube, in this manner. When your pane of glaſs is clean, dry, and warm, place a ſmall bundle of linen rags, ſuſpended by ſilk, or a ſheet or two of paper folded like a letter, or any non-electric, upon the glaſs, and bring an excited glaſs tube under the pane, and in contact with it, or near approach to it, and when it has remained there two or three ſeconds, toſs off the rags or paper, and inſtantly withdraw the tube; you will find the pane of glaſs electrified at both ſides with the reſinous power: and the ſame operation with a large ſtick of ſealing-wax excited will leave the pane of glaſs electrified at both ſides with the vitreous power; and the non-electric which was on the pane, electrified with a different power. Now to explain this matter; The two different powers of electricity being equally adheſive to the pane of glaſs (as indeed they are to all bodies) they attract and condenſe each other into almoſt an inſenſible compaſs, and in that ſtate do not exert any ſenſible action, but the atmoſphere of the excited electric drives part of the ſame power out of the glaſs into the non-electric, and at the ſame time attracts the different power of electricity from the non-electric into the glaſs; now the non-electric and the excited electric being removed, the different powers in the glaſs are rendered unequal to each other, and therefore the encreaſed power by its elaſtic force, when the attraction of the other power is leſſened, expands itſelf into an extenſive atmoſphere; which atmoſphere will act in all reſpects like the atmoſphere of any electric excited by the ſame power; for all electrics are excited in the ſame manner by the ſeparation of theſe powers, which are always equally inherent in them and all other bodies till ſeparated by friction, &c. And the reaſon that non-electrics cannot be electrified without being inſulated by electrics, is becauſe theſe different powers ſo quickly change place through non-electrics: for the atmoſphere of an excited electric always attracts one power in the non-electric, and repels the different power in the non-electric; yet when the non-electric is removed from that atmoſphere, the different powers in the non electric inſtantly rejoin by their natural attractions of each other, and become equal to each other, in which ſtate they never exert any ſenſible action. But take an iron rod of a foot long with the ends rounded off, and ſuſpend it by ſilk, and bring it within

there

three or four inches of the conductor, while the wheel is turning; then take it out of the atmoſphere of the conductor, and you will not find any ſign of its being electrified; then bring it into the ſame poſition, and you will find the two ends electrified with the different powers of electricity; the end next the conductor with the power contrary to that which the conductor throws out; which may be known by touching the ends with a piece of bog-down ſuſpended by ſilk. In the firſt caſe the iron did not receive any electricity from the conductor, nor could it receive any from the conductor in the ſecond caſe, the circumſtances being the ſame; therefore theſe different powers muſt be inherent in the iron, and muſt be thus ſeparated by the atmoſphere of the conductor, by attracting the one, and repelling the other; and in this ſeparated ſtate it is evident that each power appears in an electric atmoſphere. And it is evident, that when the iron is taken from the atmoſphere of the conductor, that theſe different powers do inſtantly reunite by their natural attraction, and are condenſed ſo as to ſhew no ſigns of being electrified. But if this iron rod was ſawed aſunder at one third of its length from the conductor, and an hole drilled in each end to receive a peg, that it may be brought as one rod towards the conductor as before mentioned, and the pieces taken aſunder while the ends were differently electrified, and carried from the atmoſphere of the conductor; each piece will remain electrified with a different power; which may be tried by the bog-down. But when theſe pieces come to touch each other, the powers are inſtantly and equally reunited through the whole, and all ſigns of electricity vaniſh.

The *Leyden* bottle ſuſpended by ſilk, may be charged, and then diſcharged, and recharged with the powers in the contrary order, only by the atmoſphere of an electrified conductor, in this manner; fix ſome ſharp pointed wires to the coating of the bottles, ſo that they project an inch below the bottom of the bottle, and fix ſome ſharp pointed needles to the rod from the inſide of the bottle projecting upward; ſuſpend the bottle by ſilk, and bring it that the rod from the inſide may point to the conductor, while the wheel is turning, and let it remain there for ſome time; and you will find the inſide of the bottle charged with the ſame power which the conductor throws out: then ſuſpend the bottle over the conductor for the ſame time, with the wires from the bottom pointing to the conductor, and you will find the bottle diſcharged; but let it remain in that poſition as long again, and you will find the bottle recharged, but in the contrary order from what it was at firſt. The conductor being electrified with the vitreous power, the atmoſphere of that attracts the reſinous power from the inſide of the bottle and from the non-electric contained therein, and itſelf enters in the room thereof; till the *inſide and non-*electric therein are electrified *like the* conductor with the vitreous power; which vitreous power within will repel the vitreous power from the outſide of the bottle, and from the non-electric in contact with it, through the points of the wires, and attract the reſinous power from the air through the points till it arrives at the outſide of the bottle; where it meets a reſiſtance from the glaſs, and therefore is accumulated there and held in firm contact by the attraction of the contrary power inſide the bottle; which contrary power it equally attracts and holds affixed to the inſide of the bottle: and thus the bottle remains charged when taken from the atmoſphere of the conductor. When the experiment is reverſed by bringing the wires from the bottom of the bottle pointed toward the conductor, the vitreous atmoſphere of the conductor attracts the reſinous power from the outſide of the bottle and the non-electric coating, and adds to the vitreous power there; which vitreous power repels the vitreous power from the inſide, and attracts the reſinous power through the needles, till the bottle becomes charged in the contrary order from what it was at firſt.

There are many caſes of this bottle, ſome of which I have mentioned in my letters, but they may be all explained (and indeed all other electrical experiments) by a due conſideration of the actions of theſe two different powers, *viz.* That they are equally attracted by all matter, and they equally attract and condenſe each other into almoſt an inſenſible compaſs when equal to each other; but when unequal to each other, the increaſed power by its elaſtic force expands into an electric atmoſphere, which atmoſphere will repel any power of the ſame kind, but equally attract any power of the different kind. But, as I have explained this matter more fully in my letters, I ſhall refer to them.

Except what I have ſaid in the following letters, I do not find any one has attempted to ſhew how any electric comes to be excited or electrified; I mean to produce an electric atmoſphere, which is very eaſily explained by the ſeparation of theſe two powers, by leſſening one and encreaſing the other; as may be ſeen in electrifying the pane of glaſs at both ſides with the vitreous power, by the excited wax; or at both ſides with the reſinous power, by the excited glaſs tube. In exciting electrics theſe powers are never entirely ſeparated, but the leſſened power acts inward to the electric, and the encreaſed power acts outward from the electric with an extenſive atmoſphere. For, when a large ſtick of ſealing-wax is a little warm and well excited, I can electrify a piece of bog-down ſuſpended by ſilk with the vitreous power, which acts inward to the wax; and again withdraw that power from the down, and leave it electrified with the reſinous power, which acts outward from the electric, only by my manner of touching the down with the wax; and a ſpectator ſhall ſcarcely perceive any difference in the manner of touching the down with the wax.

I could produce numbers of experiments, to ſhew the co-exiſtence of theſe different powers in all bodies, which every careful experimenter muſt eaſily perceive; but I believe what I have ſaid is enough to ſhew the inſufficiency of attempting to explain all the different attractions and repulſions of electricity by one ſingle medium or power. But no difficulty remains in explaining them all by the action of theſe different powers, when rightly underſtood.

Mr. *Franklin* has aſſerted that glaſs cannot receive electricity at one ſide, without parting with ſo much of its natural ſhare at the other ſide. But this is demonſtrably falſe, by the pane of glaſs being electrified equally at both ſides with the vitreous power, by the excited wax, or with the reſinous power, by the excited glaſs tube. Or, the *Leyden* bottle, ſet on an electric ſtand, will be found to be electrified at both ſides with whatever power is thrown into it.

Mr. *Franklin* has aſſerted, that when the *Leyden* bottle is charged, the power, it acts with in diſcharging, is contained in the glaſs, and not in the non-electrics withinſide and withoutſide the bottle; in which he is certainly miſtaken; for the different powers in the non-electrics are as much ſeparated in charging the bottle, as the powers in the glaſs; and, therefore, in their reuniting, on the diſcharging the bottle, they muſt add a force to the diſcharge, proportioned to the electric powers contained in them; and that will be in proportion to their quantity of matter. Which may be eaſily proved by putting a thin coating inſide and outſide the bottle, and filling another bottle of the ſame ſize with quick-ſilver, or lead and water, and a coating outſide of three or four folds of ſheet lead; the different force of theſe bottles will be eaſily diſtinguiſhed on the diſcharge. And this experiment I have tried; and here I may obſerve, that if Mr. *Franklin*'s five eight-gallon jars had been filled with the moſt denſe matter in the inſide, and an equal quantity on the outſide, I think that they would have deprived any man of his life.

I cannot forbear again expreſſing my ſurprize, that Mr. *Franklin* ſhould imagine that bodies electrified negatively (as he terms it) ſhould be without electric atmoſpheres, when all experiments ſhew the contrary. Electrify a piece of inſulated bog-down, with the reſinous power, or negatively, and you'll find all the fibres repelled in the ſame manner as they are when electrified with the vitreous power, or poſitively, as he calls it: Or, electrify the ſtream of a fountain, negatively, and you'll find it diſperſed into a very great number of ſmall particles, in the very ſame manner that it is when poſitively electrified. And from hence I made an artificial ſhower of rain, by electrifying the ſmall ſtreams of two fountains with the different powers of electricity, they were both diſperſed into very minute particles, but by the attraction of the different powers, theſe particles ran together at top, and came down in heavy drops, like a heavy ſhower of rain.

Mr. *Nollet*, when he talked of an effluence and affluence, was ſo far right; but as he knew not that it was an effluence of one power, and an affluence of the other power, it was impoſſible for him to explain the phænomena of electricity by his effluence and affluence of the ſame power. That there is an effluence and an affluence, may be proved by many experiments; but I ſhall only mention one. In one of my letters, I have mentioned the dancing of paper puppets between my table and a diſh ſuſpended from the conductor: When you dry the head of one of theſe puppets, the power thrown out from the conductor cannot enter that puppet ſo freely, as the different power which is attracted from the table can enter at the feet, which are not ſo dry; and, therefore, there will be much more of the attracted power in the puppet, than of the power thrown out, and therefore this puppet will aſcend to the diſh, and remain there; but reverſe this experiment by drying the feet and wetting the head, and the puppet will remain fixed to the table; becauſe there is more of the repulſive power in the puppet than there is of the attracted power. But when there is retained in the puppet ſo much more of the attracted power, as will balance the gravity of the puppet, than there is of the different power proceeding from the conductor, the puppet will be ſuſpended between the diſh and the table. And this may be eaſily contrived in the ſhape of the puppet; for the head, being round, does not admit the power coming out, ſo eaſily as the feet, being ſharp, admit the power going in. This experiment, with gold leaf, has been tried by Mr. *Franklin*, and many others; but they ſeem to be ſtrangers to the cauſe that produces this effect; for I do not know any one who has thought of the attracted power going into the leaf gold, &c. without which it would be inſtantly repelled from the electrified body. For it is not bare matter that is either attracted or repelled in electrical experiments; but the electric powers which are inherent or adheſive to that matter. But how theſe different powers do attract and condenſe each other, or how their atmoſpheres become repulſive of the ſame kind, I think, will not be readily diſcovered; any more than the cauſe of gravity and attraction, though the effects are evident in both.

I could go on to explain all the phænomena ariſing from electrical experiments (which I know of;) but this would neceſſarily lead me into a great prolixity, and prevent the pleaſure which experimenters have in finding out theſe diſcoveries; in which I think they cannot meet with much difficulty when they fully conſider the co-exiſtence of theſe different powers in all bodies, and their [illegible] manner of acting on each other, and that they are equally attracted by all matter.

It may now be expected that I ſhould ſay ſomething about the medicinal uſe of theſe powers; but that cannot be brought into ſo narrow a compaſs as many may imagine. For, firſt, a moderate knowledge of medicine is neceſſary to know where the effects of theſe powers may do good or harm, *viz.*, in ſhaking palſeys, and inflammatory rheumatiſms, &c. they are prejudicial. Next, an accurate knowledge of anatomy, eſpecially in paralytic caſes, is very neceſſary; for, as one ſett of muſcles revive, an equal attention muſt be had to their antagoniſts, or elſe contractions will be formed, not eaſily reſolvable afterward. And here an accurate knowledge of the action of theſe powers is abſolutely neceſſary, to know how to conduct them through the parts deſigned. I was preſſed, by a very good phyſician, who was a patient of mine, and fully convinced of the amazing effects of theſe powers in his own caſe, and many other cures which he ſaw me perform, to write ſomething ſyſtematical on the curing of diſorders by the electric powers; and carried his compliments ſo far, as to tell me, that if I let that knowledge die with me, that I ſhould be anſwerable for it in the other world. But I anſwered him, if ſo, ſo it muſt be; for that I was then ſixty-ſix years of age, which is a time men naturally grow indolent, except they are warmer in their purſuits than I was.

I may here obſerve, that ſome gentlemen, very well verſed in electrical experiments, have publiſhed their diſappointments in applying electricity to medicinal uſe. In the 48th vol. of Philoſ. Tranſ. page 786, Dr. *Cheney Hart* attempted to cure a paralytic arm of a young girl, about ſixteen; but, on being the ſecond time electrified, ſhe became univerſally paralytic; and when the additional palſey was removed by proper medicines; he tried electrifying the ſecond time; on which ſhe became univerſally paralytic again, and even loſt her voice and tongue, and with difficulty could ſwallow. This made the Doctor cautious in the farther uſe of electricity. In the 50th. vol. of Philoſ. Tranſac. page 743, Doctor *William Watſon* met with much the ſame diſappointment. In the ſame vol. Mr. *Benjamin Franklin*, at page 481, ſays that he never knew any amendment in the paralytic patients, who applied to him, after the fifth day's trial; but that they went home, and in a ſhort time relapſed.

From the great amendment, which Mr. *Franklin* ſays his patients had in five days time, I think they were all curable, if electricity had been properly underſtood and judiciouſly applied. For, tho' I have cured a great number of paralytics, (except in a few recent caſes) I ſeldom knew ſo great an amendment in five days. But ſome have continued two, three, four and five months under cure, and have found ſome amendment every day; and none of them ever relapſed. I have cured ſome, whom I really thought it was impoſſible to cure; where the limbs were waſted to ſkin and bone, and much contracted; yet, from a continued uſe of electricity, the muſcular fleſh filled up, and the limbs were reſtored to their full uſe. I cured a young woman who was univerſally paralytic, and had not the leaſt power of motion in any part but her head and neck; yet ſhe was perfectly cured, and has ſo remained for ſome years paſt. Several paralytic patients, whom I cured ten or a dozen years ago, all remain perfectly well; as all that I have ſince cured alſo do I have now under cure an old gentleman, between ſixty and ſeventy, who loſt his right ſide, and his ſpeech entirely, by a hemiplegia; in a few weeks he has recovered the uſe of his limbs, and in a great meaſure his ſpeech; and I think that he will be perfectly cured in a little more time. I will mention one rheumatic caſe, becauſe I think it particular. *Richard Seward* was fifteen years a cripple with the rheumatiſm, and all parts of his body greatly contracted, that he could ſcarcely crawl about on crutches; in about five weeks, I reſtored him the uſe of his limbs, ſo that he could walk and run almoſt as well as ever he could have done, and he has remained well for ſeven years paſt.

I have juſt hinted ſo much to ſhew that the good effects of electricity, in paralytic caſes, have been permanent; as indeed they have been in almoſt all the caſes, in which I have tried electricity; but if I were to give an hiſtorical account of all the cures I have performed, merely by the powers of electricity, it would make a pretty large book; I muſt therefore here deſiſt.

As I have often ſaid the electric powers condenſe each other, and have given ſome experiments which I think ſufficiently ſhew that they do ſo; yet it may not be amiſs to add a few more experiments to make it more evident to others.

Take two panes of clean ſaſh glaſs, which will lie evenly on each other; place a ſheet of paper, folded like a letter, on your table; place the two panes on the paper; and a like paper on the upper pane; then take a large glaſs tube excited, and roll your tube over the upper paper; repeat the exciting, and rolling the tube five or ſix times; at the laſt rolling take up your panes, upper paper and tube together, and immediately take off the tube and upper paper; then bring the two panes, joined together, to a piece of bog-down ſuſpended by ſilk; you will find the panes, thus joined, have but very little effect on the down; but ſeparate the panes, and each pane will be ſtrongly electrified, the upper pane with the vitreous power on both ſides, and the under pane with the reſinous power on both ſides: touch the down with either pane, and it will be repelled by that and attracted by the other. Place your panes together, as at firſt, and you will find that the powers condenſe each other, ſo as to have but little effect on the down: but as you ſeparate the panes you will find the different powers to act as before. The panes, if they lie in contact with each other and are highly electrified, will ſtick together by their mutual attraction, and ſeparate with a ſnap. If this experiment is made on a pewter plate placed on a glaſs ſtand, you will find that the powers have changed place through the whole; the vitreous power of the tube attracting part of the reſinous power from the firſt glaſs, and adding part of its own vitreous power to the glaſs; by which means the glaſs becomes electrified with the vitreous power; which vitreous power repels the vitreous power of the under glaſs into the paper and plate, and attracts the reſinous power from the paper and plate into the under glaſs; and thus the under glaſs is electrified with the reſinous power, and the plate with the vitreous power. A due conſideration of this experiment may explain all the tranſactions of the electric powers. For in exciting any electric the rubber always draws off as much of one power as it adds of the other, and is therefore always electrified with a power contrary to the encreaſed power of the excited electric: and when an excited electric electrifies an inſulated non-electric, it draws as much of one power from it as it gives of the other; and therefore is itſelf as much unelectrified, as it electrifies the non electric. And thus this change goes on till the powers become equal to each other; which in non-electrics is done almoſt inſtantaneouſly.

As I have ſaid that the magnetic powers act by the ſame laws as the electric powers do, (except that they do not change place from magnet to magnet, as the electric powers do from one body to another ;) this may be ſeen by placing two artificial magnets ſide by ſide, with the north and ſouth pole joined together ; in which poſition the powers attract and condenſe each other, ſo as to have but little effect on any iron applied to them. Apply two large magnets, ſo placed to a ſmall key, and they will not take it up ; but ſeparate them but half an inch, and they will take it up ; but when you cloſe the magnets the key drops from them ; which ſhews that the powers then condenſe each other.

To ſhew that while there is an efflux of one power of electricity, there is alſo an afflux of the other power of electricity, when any non-electric matter is placed ſo near and in ſuch circumſtances as that it can be drawn therefrom. Take a ſharp pointed needle four or five inches long, ſupported on a point like a compaſs needle, and turn half an inch of each point at right angles with the ſhank, but contrary to each other, and parallel to the horizon. This needle, when placed upon the conductor while the wheel is turning, will circulate with great velocity, but always from the points, becauſe the electric fire flying off from the points acts forcibly on the air, and is conſequently re-acted on, which occaſions this motion. But hold this needle three or four inches under the conductor, and it will turn in the ſame manner, by a ſtream of electricity, of a contrary power to that thrown off from the conductor, which is drawn in from you, and delivered from the points of the needle to the conductor. Let this needle be placed on a glaſs ſtand at the ſame diſtance under the conductor, and it will not move, becauſe no electricity can be drawn through it; but hold a pin within an inch of the non-electric pedeſtal, and the needle will immediately begin to turn; a ſufficient quantity of electricity being drawn from you through the pin.

This ſtream of a contrary power from all pointed or ſmall wires held in the hand, and in the atmoſphere of the conductor, while the wheel is turning, is evident; but placed on, or in electrics the effect ceaſes.

In my letters I have ſaid that the ſphere of activity of the electric powers is increaſed by heat; which may require ſome explanation, and is very neceſſary to be underſtood in the rarefaction and condenſation of vapour, &c. and even of the air; for it is evident that every particle of the air is attended by the electric powers; becauſe we can electrify the air with which ever power we pleaſe, and that either power can be drawn from the air in many experiments. From hence many properties of the air may be explained; *viz.* its electricity, power of refracting light, conveying ſounds, entering into denſer fluids, &c. which I ſhall not now undertake. When I ſay that heat increaſes theſe powers, I do not mean that heat ſeparates or excites them to attract and repel other bodies by an exchange of powers; as I have explained that all electrifying is performed; but that heat dilates and expands them by rendering them leſs denſe, and conſequently more extenſive in themſelves. This is evident to me in many experiments, but if I were to recite them all I may get beyond the bounds of my readers patience. Take a glaſs tube and warm it well by the fire, and when excited by friction, bring it under a piece of bog-down ſuſpended by ſilk, at the diſtance of two or three inches, and then touch the down at the top with your finger, and withdraw the finger and tube at one time, the down will be ſtrongly electrified with the reſinous power; then ſtrike your tube up againſt the down, you will find the down will ſtick cloſe into the tube, and the fibres riſe one by one ſlowly till the down becomes electrified with the vitreous power, and is conſequently repelled from the tube. But try this experiment with a tube quite cold, and you cannot eaſily make the down ſtick to it; which ſhews that the reſinous power, which acts inward to the tube, is much more dilated when the tube is hot, than when the tube is cold. This experiment may be better tried with a pane of glaſs.

I believe that it is unneceſſary to ſay that the rarefaction of vapour, &c. muſt follow when theſe powers are dilated by heat, or a condenſation when they are contracted by cold; ſince it is evident that the particles of vapour, &c. are kept at a diſtance by theſe powers. But this explains a matter which I think has hitherto lain in the dark, *viz.* The cauſe of rarefaction in vapour, &c. by heat, and condenſation by cold. And may alſo explain how vapours may aſcend in a hot day, and deſcend in dew in the cool of the evening and at night, without any exchange of the electric powers in the particles of dew. And may alſo explain many other things, too prolix to enter upon in this place.

Since electricity does really conſiſt of two different diſtinct elaſtic mediums or powers, which co-exiſt in all bodies, and act in the manner which I have ſet forth in the following tracts; and that Mr. *Fraklin* and all his followers continue to attempt to explain the effects of electricity by one ſingle power; I muſt beg leave to examine Mr. *Franklin*'s pretenſion to the great honour which he has received for diſcovering the cauſe of thunder; which I claim as due to me. For if electricity conſiſts of two different powers, it is impoſſible to explain the effects by one power; and if electricity is the cauſe of thunder, it is as impoſſible to explain that cauſe without an accurate knowledge of electricity.

I have already ſhewn that Mr. *Franklin's* principles are fallible and inſufficient, and far from being founded on that evident truth which ought to conſtitute principles; and that his opinions and aſſertions, about the operations of electricity in and on glaſs, are demonſtrably falſe, and that his whole doctrine of electricity depends upon the action of one ſingle medium or power, which never can explain the phænomena which ariſe from electrical experiments.

In his attempt to explain the cauſe of thunder, he ſays that clouds ariſing from the ſea are highly electrified, but that clouds ariſing from the land are but little electrified; but ſurely his method of collecting electricity (as he terms it) to electrify vapour ariſing from the ſea, is a very extraordinary one.

He ſays, the ocean is a compound of water, a non-electric, and ſalt, an electric perſe. When there is a friction among the parts near the ſurface, the electric fire is collected from the parts below.

I believe that every one verſed in electrical experiments muſt think this notion abſurd, and unworthy of ſo ingenious a man, though it is the baſis of his hypotheſis. For I am certain that electricity never was excited by whirling a glaſs globe in a tub of water. As to his opinion about the aſcent of vapour; he ſuppoſes that the particles of vapour are attached to the particles of air, and that they aſcend by that means; in which I think that he is entirely miſtaken. For, if water, which is a thouſand times heavier than air, be joined to air, it will certainly make the mixed heavier than the circumambient air; and therefore the air muſt rather deſcend with the water, than the water aſcend with the air. Every old woman, who has made pickle, knows that an egg will ſink in clean water; and that, when a quantity of ſalt is diſſolved in the water, the egg will ſwim. And this may ſerve as an anſwer to a late treatiſe, where the author, from a ſolution of metals in proper menſtrua, would prove that a ſolution of water in air, is the cauſe of the aſcent of vapour, &c.

I now find from Mr. *Prieſtley*'s hiſtory of electricity, that an analogy between lightning and electricity, was very early thought of, even before Mr. *Gregg*'s time, and very often ſince his time. If Mr. *Franklin* had any hint of ſuch a conjecture, his propoſed method of proving the truth thereof was very rational. For if electricity was the cauſe of thunder, the only place he could ſearch for it, muſt be in the clouds; and from his acquaintance with electrical experiments he muſt have known that metallic rods were the beſt conductors of the electric power; and therefore, by placing ſuch rods upon electrics, as near the clouds as poſſible, was a probable method of trying whether ſuch rods would be affected by electricity.

But this experiment, propoſed by Mr. *Franklin*, was firſt tried by Mr. *D' Alibard*, Mr. *De Lor*, Mr. *Le Monnier*, and the Abbe *Nollet*, and others in *France*, and by others in different places; but, to the preſent time, neither Mr. *Franklin*, nor any of theſe experimenters have ſhewn how theſe metallic rods became electrified, or how electricity could be the cauſe of thunder. Nor can this matter ever be explained, by thoſe who ſuppoſe that electricity conſiſts of but one ſingle elaſtic medium or power.

That electricity may be found in the clouds, is certain; for it attends all the matter in nature, which we are acquainted with; and, that the clouds may electrify iron rods, &c. in near approach to them, and placed upon electrics, is true; but this is far from ſhewing, proving, or explaining that electricity is the cauſe of thunder. For, if only a communication of electricity from one body to another can be ſaid to be the cauſe of thunder, it may be ſaid that the electricity of the human body is the cauſe of thunder; as it will electrify other matters: Witneſs Mr. *Symmer's* black and white ſtockings. Therefore, a diſcovery that the clouds are electrified, is not a diſcovery of the cauſe of thunder; nor can it be explained how electricity in the clouds can be the cauſe of thunder, by any thing which Mr. *Franklin*, or others have ſaid about poſitive and negative electricity, or a plus and minus of the ſame power, or that glaſs throws out this power, and that wax, reſins, and ſulphur, &c. drink it in; nor by any thing Mr. *Franklin* has ſaid about charging, or diſcharging the *Leyden* bottle, where he ſuppoſes that as one ſide of the bottle receives electricity, the other ſide parts with an equal quantity; and, when a communication is made between the ſides, that the emptied ſide greedily drinks up the abounding electricity from the other ſide. Suppoſing this doctrine to be true, I aſk any thinking man, how he can from thence prove that electricity in the clouds can be the cauſe of thunder? For, tho' the *Leyden* bottle gives the ſtrongeſt indication of an analogy between electricity and lightning, yet there are not any bottles, or glaſs, in the clouds, to produce this effect, by having one ſide filled and the other emptied. And Mr. *Franklin* aſſerts, that the power of the *Leyden* bottle is lodged in the glaſs, and not in the non-electrics within and without the bottle. How, then, does his increaſing the force of this bottle, by adding a number of bottles together, give us any proof that electricity in the clouds is the cauſe of thunder? Or, if electricity is but one ſingle medium or power, as he aſſerts, by what known principle of motion is that power put into action? Or, what is the manner of that action, to produce thunder?

I could eaſily take Mr. *Franklin*'s doctrine of electricity to pieces, and ſhew that it is impoſſible, from the whole, or any part thereof, to prove that electricity is the cauſe of thunder; but I believe that it is ſufficient to ſay that he has never thought of two different diſtinct powers exiſting in electricity, or their co-exiſtence in all bodies, or their manner of acting on each other; which all experiments ſo clearly demonſtrate, that I am ſurprized that ſuch an ingenious man, after all his experiments could have miſſed this diſcovery; without which it is impoſſible to explain the cauſe of thunder; or any effect ariſing from electrical experiments.

Mr. *Franklin's* metallic rods, placed from the top of high buildings to the earth, may probably prevent the injuries of thunder, by conducting the electricity of the cloud to the earth; but as he ſeems ignorant that an equal quantity of a different power of electricity muſt be conducted from the earth to the cloud to produce thunder, it ſhews that he is not fully acquainted with the uſe of theſe rods, or the cauſe of thunder; for the electric powers are never rendered viſible, except in their paſſing from one body to another, in oppoſition to each other; when they condenſe each other into the form of flame, and perform many of the effects of fire, but more inſtantaneouſly than any fire which we are acquainted with. And I am really ſurpriſed to find that ſuch a number of ingenious men, after a courſe of experiments for ſo many years, have not diſcovered the co-exiſtence of theſe different powers of electricity in all bodies, and their manner of acting on each other. Without which knowledge it is impoſſible fully to explain any effect ariſing from electrical experiments, particularly the moſt forcible ones, ſuch as thunder; for theſe are cauſed by the great attraction and condenſation of the different powers in paſſing in oppoſition to each other from one body to another. And if it be duly conſidered that in all excitation and communication of electricity, there muſt be a reciprocal exchange of the different powers to each body; it will explain all the phænomena of electricity which I know of. And to this ſhort rule have I reduced it, after all that has been ſaid about it. For the rubber takes as much of one power from the electric, as it gives of the other; by which means the rubber and electric become equally electrified with the different powers. And the ſame exchange is made in all communication of electricity.

Mr. *Franklin* and all his followers were led into error about the impermeability of glaſs, and the manner of charging a bottle, by his obſerving that when a ſpark was taken into the bottle, an equal ſpark may be taken from the outſide; which made him imagine that there was ſome ſtratum in the middle of the glaſs, which prevented the paſſage of electricity, and that as one ſide was filled the other ſide was emptied; and that when a communication was made between the ſides, the emptied ſide greedily drank up the redudancy of the filled ſide. But had he known of the two powers, and the rule I have now given for their changing from body dy to body, he muſt know that when a ſpark of the vitreous power is taken into the bottle, an equal quantity of the reſinous power goes from the inſide of the bottle to the conductor; by which means the bottle is electrified with the vitreous power; but when you apply a non-electric to the outſide of the bottle, part of the vitreous power goes to the non-electric in exchange for an equal quantity of the reſinous power drawn to the outſide of the bottle, which reſinous power is held there by the attraction of the vitreous power on the inſide of the bottle. And thus this exchange of powers goes on as far as the powers can be ſeparated between the globe and cuſhion.

Place a charged bottle upon an electric ſtand, and let a piece of bog-down, or cork ball, &c. ſuſpended by ſilk hang againſt the outſide of the bottle; when you touch the outſide with a finger, the down, &c. remain quiet by the ſide of the bottle; but then touch the top of the bottle and the down flies off ſtrongly electrified with the reſinous power; and thus you may go on for a great number of times to alter the balance of the powers withinſide and withoutſide the bottle, by alternately touching the top and bottom. Nor can Mr. *Franklin* or any gentleman ſuppoſe that it is the return of his plus or poſitive power to the emptied pores of the bottle, which electrifies the down minus or negatively? But the truth is, that when you touch the top you take a ſpark of the vitreous power from the inſide, and in exchange give as much of the reſinous power to the inſide; which leſſens the attraction of the vitreous power withinſide the bottle, which leaves the reſinous withoutſide in greater quantity than the vitreous withinſide, and conſequently at liberty to exchange powers with any non-electric in contact with it; and thus the down, &c. become electrified with the reſinous power. Or take two ſheets of paper folded like large letters, and place them upon your table, with a pane of clean well dried glaſs on each paper; then take a large glaſs tube, well excited, and roll it over one of the panes, then take up the pane and tube together, and inſtantly withdraw the tube; you will find that pane ſtrongly electrified at both ſides with the reſinous power; or negatively and minus, according to Mr. *Franklin*'s terms; which he ſays is a thing that cannot be done. See his letters, printed by *R. Cave*, page 71, 72. ſecond edition. Repeat this operation, and while your pane is electrified with the reſinous power, lay it on the other pane for four or five ſeconds, then take up your panes together, and then ſeparate them; you will find the firſt pane ſtill electrified with the reſinous power, or minus, and the other pane electrified, at both ſides with the vitreous power, or plus. Now if Mr. *Franklin*, or any other man, can frame or ſtrain an hypotheſis to explain, by the action of one ſingle power, how a body, diveſted of its natural ſhare of that power, can give another body a ſuper-natural or additional ſhare of the ſame power; I think they will overſet all the rules of reaſoning which I know of; for I think it is a confirmed maxim, that no body can give that, which it has not. But this matter is eaſily explained by the doctrine I have all along given; the firſt pane, having a greater quantity of the reſinous power, repels the reſinous power from the ſecond pane, to the non-electric under it, and attracts an equal quantity of the vitreous power from the non-electric into the pane; by which means that pane has an encreaſed ſhare of the vitreous power, which encreaſed power will, by its own elaſtic force, expand itſelf into an extenſive atmoſphere, and therefore the pane will be electrified by that power.

Some gentlemen I think have reaſoned themſelves out of their ſenſes, and denied the exiſtence of electric atmoſpheres, and ſaid that Mr. *Franklin*'s doctrine would do better without them. But I would aſk theſe gentlemen a civil queſtion, whether it is mere inanity which knocks down ſteeples and towers, rends trees, tears up the earth, kills men and cattle, ſets places on fire, &c. or I might ſhorten the queſtion by aſking how mere inanity or nothing can act? but this would be a diſpute about nothing.

If gentlemen had been content to confine themſelves to facts, I mean ſuch effects as are evident to our ſenſes, I think that they muſt have neceſſarily diſcovered the exiſtence and co-exiſtence of theſe different powers in all bodies, and their manner of acting on each other, and their being acted on by all matter; which would have prevented their running into ſuch vague conjectures and ſtrange ſtrained hypotheſes to explain theſe effects by a ſingle medium or power; while it is ſelf-evident that two different mediums or powers do really exiſt.

If what I have ſaid already is not ſufficient to prove the exiſtence and manner of acting of theſe different powers, I ſhall always be ready to give any farther explanation, in my power, to any unprejudiced enquirer, who thinks it worth the while to apply to me about what he thinks doubtful.

In one of my letters I have confeſſed how ſlender an acquaintance I had with electricity, when I wrote an hypotheſis to ſhew the cauſe of thunder; for that I had never read a line or converſed with any one on, or about electricity; but that hypotheſis, and another on the aſcent of vapour, &c. being accepted by the Royal Society; and the only one that ſtands recorded in the Philos. Trans. and, as far as I can yet judge, as correct as any thing publiſhed on that ſubject; as far as electricity was then underſtood, or from any thing I have ſince ſeen publiſhed about it; I was encouraged to apply myſelf to electrical experiments, and to write the following tracts; in which I think that I have demonſtrated that electricity is the cauſe of thunder, as far as the nature of the proofs can poſſibly admit; and have given a clear and eaſy doctrine of electricity and magnetiſm; which will fully explain all the effects, which I know of either. I have often ſince ſeen and ſhewn, to gentlemen in company with me, clouds approaching each other with an encreaſed velocity, by their mutual attraction, and could foretell to them, to a few ſeconds, when the flaſh would happen.

It would be an endleſs taſk to confute all the falſe reaſonings, and miſapplied experiments which I have ſeen publiſhed about electricity; and therefore I muſt here deſiſt.

But, as I have objected to ſome of the principal writers on electricity, I ſuppoſe that their partiſans will object to what I have wrote; but I ſhall not trouble myſelf with a reply, 'till I meet with ſomething more explicit and intelligible on electricity, than any thing which I have yet met with.

I readily grant that they may object to my manner of writing; becauſe I never took a note of what I intended to ſay, nor ever ſtruck out a line which I did write on that ſubject; except a few in the beginning, where I had ſaid that electricity may be the æther of Sir *Iſaac Newton*, before I was ſufficiently informed

formed to ſhew how it may anſwer his purpoſes. I know that ſome gentlemen have taken the characteriſtics of his æther from a late writing of that great man; where he endeavours to make it the cauſe of gravity; but I have ſhewn, in my letters, that ſuch an æther, as he there ſuppoſed, could not have anſwered any of his purpoſes. I now think that the electric powers, when properly underſtood, will appear to be that ſubtile medium which ſupervades the ſurfaces of all bodies, and will be found ſufficient for the reflecting and refracting of light, and more fitted for that purpoſe, than any medium Sir *Iſaac Newton* could have poſſibly ſuppoſed; for it was not poſſible for the then known powers of nature, or laws of motion, to ſuppoſe the exiſtence of two ſuch ſubtile elaſtic mediums or powers, acting in the manner which they do on each other, and their being equally acted on by all matter, 'till this knowledge was deduced from their effects; which in his time were very little known; and to the preſent (by all I have ſeen publiſhed) but very little underſtood.

As to the matter of what I have wrote, it appears true to me, and I believe will do ſo, to any unprejudiced reader, who will give himſelf the trouble diligently to enquire into it.

My reaſons for publiſhing theſe papers are, in the firſt place, that they will ſave a great deal of trouble to thoſe who have a mind to enquire into the electric powers, and their general laws of action; and then to appeal to the unprejudiced part of mankind, whether thoſe gentlemen of the Royal Society, who call themſelves electricians, have done themſelves honour, or me juſtice, in ſuppreſſing, and not publiſhing theſe tracts, which were addreſſed to the Royal Society. By which means, they have given Mr. *Prieſtley*, one of their own fellows, an opportunity of ſtealing my doctrine, and making it his own, by way of hypotheſis, in his hiſtory of the preſent ſtate of electricity; though it is plain, from what he wrote, preceding and conſequent to it, that he did not underſtand it; which I could eaſily point out through the whole. They have alſo defrauded me of the credit juſtly due to me, for the diſcovery and full explanation of the cauſe of thunder; and for a clear and eaſy doctrine of electricity and magnetiſm, &c. as contained in thoſe tracts. The truth of which doctrine has been verified to me by a very great number of experiments, and a continued application of the electric powers to various purpoſes, for many years; particularly in medicinal uſe; by which I have performed many cures in paralytic caſes, which our phyſicians here thought miraculous; and, in truth, they appeared to me in the ſame light; if a miracle be that, which is above our knowledge of the powers of nature. I know that miracles have been defined to be above or contrary to the laws of nature; but I think that nothing can be above or contrary to theſe laws, which were eſtabliſhed invariable, by a Being of infinite Power, Wiſdom and Goodneſs to his creatures.

Liſmore, in *Ireland*, 1769.

⁂ Theſe papers are not to be conſidered as one work, but as the gradual invention of the author, as they are all his own; for every body was then fond of being firſt to publiſh their new diſcoveries in this ſcience of electricity.

PHILOSOPHICAL ESSAYS, &c.

LETTER I.

Liſmore, 20th *Sept.* 1751.

GENTLEMEN,

THE greateſt men of moſt ages having thought it worth the while to enquire what was the cauſe of thunder, and the world ſeeming to acquieſce in an hypotheſis ſubſcribed by ſome great modern names ; it muſt appear preſumptuous in me, to offer you ſome thoughts for a theory intirely new, (at leaſt it is ſo to me) without I can ſhew that the former hypothſes are ill grounded, and far from being ſatisfactory. In order to which I ſhall only object to the lateſt (to avoid prolixity) which now has the general conſent.

I think the baſis that this hypotheſis ſtands on, is the authors aſſuming an analogy between thunder and fired gunpowder; and then proving that there are ſulphureous and nitrous particles in the air, they leave them to take fire by fermentation or ſome other accident, and from thence to form thunder.

Firſt, the analogy is not juſt; for there is not any thing ſimilar to thunder in fired gunpowder, except the noiſe. Which may be ſhewn from the different direction of their fire, and their very different effects.

Fired gunpowder acts, from a center to a circumference, with equal force at equal diſtance every way, by propelling the circumambient air by the exploſion it makes. The fire of thunder acts in rectilinear angles (as I have ſeen, and as any body may who will obſerve it) with ſuch ſubtile and diſtinct effects, as cannot be explained or imitated by the fire of gunpowder. The hiſtory of which effects is too well known to need a repetition here.

I ſhall go on to ſhew ſome inſuperable difficulties in the formation and firing this ſuppoſed aerial gunpowder. And firſt I think it inconceivable that the ſulphureous and nitrous particles ſhould coaleſce with ſome other unknown third body, in the place of charcoal, in ſuch exact proportion as is neceſſary to make gunpowder of any perfection, and to form a body compact enough to equal the noiſe of thunder, when fired in the open air. For ſuch a body muſt neceſſarily deſcend by its own gravity long before it arrives to a bulk ſufficient for the purpoſe. And ſecondly, I think it contradictory to all experience, that ſuch a coaliſion of nitrous particles ſhould ever happen in the common ſeat of thunder; which is in the moſt collected ſhowers which deſcend; for there the nitrous particles muſt be abſorbed and diſſipated in the water; in which ſtate I think it impoſſible for them to take fire.

Theſe and many other conſiderations (too prolix for the compaſs of a letter) induced me to ſearch for ſome other cauſe of thunder, which I think I have diſcovered in that fire which is made apparent in electrical experiments.

This fire pervades and adheres to moſt bodies, while it flies and cannot be brought to mix with ſome particular bodies. I ſhall here only mention two; air which it flies and ſhuns, and water which it more intimately pervades, than almoſt any other body. I muſt alſo obſerve that this fire does not only pervade bodies, but that it ſurrounds and covers them to a certain diſtance from their ſuperficies in proportion to the ſtate of its activity; which is encreaſed by heat. And that when it is artificially or accidentally protruded upon any body beyond its natural affection, it will fly off to the next approaching body, which is not ſo much impregnated with this fire; and when it departs in any conſiderable quantity, it makes a conſiderable noiſe or crack. All which is demonſtrated by electrical experiments.

Now to ſhew that this fire is the real cauſe of thunder, we need only conſider it attending every veſicle of humid vapour riſing into the atmoſphere, and covering its ſuperficies to a certain depth, which I think it muſt certainly do. I ſhall not here ſpeak my opinion how far this fire is the cauſe of vapours aſcending, becauſe I ſhall trouble you with that hereafter.

Having got the vapour aloft attended by this fire, without aſſigning any cauſe for its aſcent, ſo without aſſigning any cauſe for its deſcent, I ſhall let it come down as uſual, which is in drops much larger than the veſicles in which it aſcended. Now in the coaliſion to form theſe drops, we muſt conſider what becomes of our fire; for the ſurface of theſe larger drops increaſing only as the ſquares, but their ſolids as the cubes of their diameters, the fire which ſurrounded the ſuperficies of the veſicles muſt be protruded to a much greater diſtance from the ſuperficies of the larger drops, and by that means made more in proportion to the larger drops than its natural affection would have made it join them with, and conſequently rendered more apt to fly off to the next approaching or approached body, not ſo fully impregnated by this fire.

I have obſerved before that the conſtant ſeat of thunder is in thoſe clouds which are moſt compact of humid vapour, and which deſcend in the heavieſt ſhowers, and that generally in warm weather, when the adjacent atmoſphere is ſerene; ſo that the humid vapours are almoſt all collected into this chain of clouds, where according to the compaction, there will be a body of this fire collected (ready to fly off) ſufficient to perform the greateſt effects of thunder. Which may be eaſily computed from the force of electrical experiments; where the ſmalleſt portion of this fire, flying off from an electrified body, makes an audible crack, and is able to give a conſiderable ſhock. What then muſt be the force of this fire when it is ſo collected as to break from a cloud in a body of fire two or three hundred yards in length? which I have often ſeen.

Now ſome of theſe clouds coaleſcing in their deſcent, and the drops increaſing in their magnitude, there is a vaſt body of this fire collected more than what would naturally adhere to theſe drops and their ſurfaces; which being rendered more active in its vibrations by the heat of the lower part of the atmoſphere, the ſphere of its affection (pardon the word, for I have no other) is alſo increaſed in proportion to the body of fire, which enables it to fly off to clouds (not ſo much impregnated) at a conſiderable diſtance, with that violent crack, ſo much taken notice of, tho' it is far from being the moſt wonderful of its effects; the dire influence of which we often happily eſcape, by this body's being diſſipated by the heat of the lower atmoſphere, before it comes within the ſphere of its affection for bodies on the ſurface of the earth.

There is a ſubſequent rumbling noiſe heard after the firſt crack or cracks of thunder, (for this fire does not all break off from one point) which has been taken notice of and oddly accounted for; but I think it neither is or can be more than echoes from adjacent clouds, which at this time are generally denſe enough for that purpoſe; and the noiſe growing fainter in proportion to the times of its being returned, I think ſufficiently proves it.

As to the ſubtile effects of thunder, I ſhall leave you to compare them with thoſe of electricity, only allowing for the different force of fire, which is ſo much greater in thunder than can poſſibly be procured from artificial experiments; and I believe the analogy will plainly appear. I ſhall only hint that where one body has been injured by thunder, and another (tho' in contact with it) has remained untouched, that the latter will be found to be of that kind which electrical fire will not join with.

I muſt beg you will let me know whether this theory is worth your acceptance, for I fear that I am like a fond mother, blind to the imperfections of my own child. I have dandled this opinion theſe eighteen months paſt, ſtill fearing to lay it before you. And now inſtead of ſeeing its defects, I begin to fancy that it has the face of truth and demonſtration. If you think this diſcovery worth the purſuit, I ſhall venture to trouble you hereafter with ſome farther attempts, to ſhew that this fire is a moſt conſiderable agent in nature.

Firſt, that the aſcent of vapour and exhalation is principally owing to it, and that our atmoſphere by that means is kept more homogeneal than is generally ſuppoſed, and fitter for reſpiration, viſion, &c. and that clouds of heterogeneous matters are kept ſuſpended at their uſual height merely by this fire.

Secondly, I ſhall prove that this fire is the cauſe of reflection, refraction, and inflection of light.

Thirdly, I ſhall endeavour to ſhew that this fire is the cauſe of that ſecondary attraction and repulſion, that Mr. *Newton* has taken notice of.

Laſtly, I ſhall give ſome hints of the great uſes of this fire in animal life and vegetation. What further I have thought of this fire I ſhall not now trouble you with. I am,

GENTLEMEN,
Your moſt humble and
moſt obedient ſervant,

HEN. EELES.

⁂ Sent this letter dated 18th June, 1752, to the Preſident and Council, of the Royal Society in London.

The

The ANSWER to the Firſt Letter.

SIR, *London*, *Nov.* 25th, 1752.

YOUR letter, of the 18th of *June* laſt, to the Royal Society, concerning the cauſe of thunder, came to my hands after their ſummer receſs, ſo that it could not be preſented to them 'till after the reſuming of their weekly meetings this month; on the 16th of which I read it to them, and received their orders to return you their thanks for it, and their deſire of your thoughts upon the other ſubjects which you mention.

Your hypotheſis, with relation to thunder, is juſtified by the experiments made in *May* laſt, and ſeveral times ſince, upon thunder-clouds in *France*, and verified by others in *England*; of which the world will have full ſatisfaction in the volume of the Philoſ. Tranſactions for the preſent year, now in the preſs, and to be publiſhed about *February* next.

Any of the future communications, with which you ſhall favour the Society, will find me at my houſe in Norfolk-ſtreet, in the Strand, or at that of the Society, in Crane-Court, Fleet-ſtreet. I am,

SIR,

Your moſt obedient humble ſervant,

THOMAS BIRCH,
Secret. RS.

To Henry Eeles, eſq;
at *Liſmore*.

LETTER II.

SIR, *Liſmore*, 10th *Nov.* 1754.

I RECEIVED your letter of the 25th *Nov.* 1752, in anſwer to my letter to the Royal Society, concerning the cauſe of thunder. As you tell me that they have honoured me with their deſire of my thoughts on ſome ſubjects mentioned therein, I have ventured to ſend you a letter for them, on one of theſe ſubjects, incloſed in two packets, by this poſt.

I fear that the Society will think my letter too hypothetical; our great modern philoſopher having in ſome meaſure condemned the uſe of hypotheſes. But I muſt boldly obſerve that, that objection is made in a part of his works which is entirely hypothetical; I mean his Queries at the end of his third book of Optics. And I ſhall venture to ſhew (if the Society will have patience to hear me) that there is a great deal hypothetical in his ſecond book of Optics; where he thinks himſelf more aſcertained. For he there ſpeaks pretty poſitively of four cauſes for the refraction, &c. of light; three of which muſt be wrong, and the fourth, (his æther) in the manner he has propoſed it, can never be ſufficient for the purpoſe. Not that I intend impudently to blame that truly great man, but to endeavour to inveſtigate the cauſe of reflection and refraction of light, by means not made ſenſible to mankind in his time.

I may alſo obſerve, that, if his hypotheſis had not preceded his calculation, the world had never been obliged to him for his great doctrine of attraction. For at firſt he only ſuppoſed the power of gravity may extend to the moon; and that ſhe may be retained in her orbit thereby; but when he made his calculation, (having miſtaken ſixty *Engliſh* miles for the meaſure of a degree on the ſurface of our earth) he thought that ſome other power muſt co-operate with that of gravity, to keep the moon in her orbit; and, for that time, laid his attempt aſide; 'till *Picart*, in *France*, meaſured a degree, and found that it contained about ſixty-nine *Engliſh* miles and an half; and then, or ſome years after, our great author, having reſumed his computation, found that the power of gravity was alone ſufficient to keep the moon and planets in their orbits; and thereby proved his hypotheſis.

Indeed, in works of invention, I can't ſee how it is poſſible to proceed without ſomething hypothetical; for, the ſuppoſition muſt ever precede the proof. I am not for eſtabliſhing hypotheſes as principles; but if an hypotheſis is rationally founded, I think it is worth the while to enquire into the truth of it, and thus invention may proceed. I ſhall not ſay any thing in juſtification of my own, becauſe I have not any vanity to gratify; for, if my letter has nothing worth their, or your acceptance, I can moſt readily and willingly aſk pardon for the trouble I have given, and deſiſt from giving any for the future. I am,

SIR,

Your moſt obedient,

humble ſervant,

HEN. EELES.

To the Rev. Mr. Thos. Birch, Secretary to the Royal Society, in Crane-Court, Fleet-Street, *London*.

LETTER III.

GENTLEMEN, *Liſmore*, 16th *Nov.* 1753.

THE great honour you have done me, in accepting my letter concerning the cauſe of thunder, has emboldened me to proceed, in part of what I therein propoſed, *viz.* to endeavour to ſhew, that the fire, which is made apparent by electrical experiments, is the principal cauſe of the aſcent of vapour and exhalation; and that the lower part of our atmoſphere is, by that means, kept more homogeneal, than is generally ſuppoſed, and fitter for reſpiration, viſion, &c. and that clouds of heterogeneous matters are kept ſuſpended at their uſual height, merely by this fire.

To which I have added ſomething, concerning the cauſe of the winds; and to explain the general phænomena of the weather and barometer.

But, as I muſt now contradict ſome generally received opinions, it may be thought decent to make ſome apology for offering my own; but I ſhall not take up your time in ſo doing; for, if they carry an appearance of truth, I need not make an apology for them; if they appear falſe, they cannot injure thoſe opinions which they contradict; and then I have but one apology to make, which is, to acquieſce, and ſincerely aſk your pardon for having troubled you with them.

It is agreed that the aſcent of vapour and exhalation through the air may be effected two ways; by impulſe, and an alteration of their ſpecific gravity.

That vapour does not generally aſcend by impulſe, may be proved by many familiar experiments, *viz.* Put boiling water into a veſſel, and then empty it, and hold the veſſel with the aperture downwards; the vapour which is afterwards expelled from the veſſel muſt be in a direction downwards; but we find that, as ſoon as it has got but a very little below the rim of the veſſel, it has its direction altered, and aſcends by the laws of ſpecific gravity. The ſame thing may be obſerved in all boiling veſſels where the vapour is emitted in a direction downwards; or in cold weather, when the vapour of a man's breath may be ſeen, let him breathe downwards, and the direction of his breath will be preſently altered, as in the former caſe.

Since vapour does then aſcend without any other impulſe, than that which is incident on all bodies, aſcending by the laws of ſpecific gravity, it is neceſſary to enquire how the ſpecific gravity of vapour is altered to cauſe its aſcent.

This is generally ſuppoſed to be done, by filling veſicles of water with rarified air, until the diameter of the veſicle be ten, or more times, the diameter of a drop compoſed of the ſame conſtituent particles of water; and that the veſicle by this means becomes ſpecifically lighter than air.

But I think that this cannot be done ſo eaſily as it has been generally imagined; and, when done, it will not be ſufficient for the purpoſe: which will appear from the following conſiderations. Firſt, the great difficulty in forming thoſe veſicles, eſpecially of the particles of dry bodies, carried off by exhalation, and filling them with rarified air, while the exterior air remains condenſed. Secondly, that there is not any allowance made for the weight of the included air. Thirdly, the conſtituent particles of water are but very little (if at all) altered in their ſpecific gravity. Fourthly, that this thin veſicle can never be a ſufficient boundary between the exterior condenſed air and the interior air ſo exceedingly rarified. And therefore as the denſity of air is proportional to the force compreſſing it, it follows that the interior air muſt be ſuddenly compreſſed, and the veſicle with it, until it be of an equal denſity with the exterior air; in which ſtate the veſicle is not more fitted for aſcending, than if the ſame conſtituent watry particles were formed into a ſpherical drop.

For here I muſt beg leave to ſet aſide a falſe reaſoning which I have found in many approved ed writers on this ſubject, who aſſert that the ſpecific gravity of bodies is altered by a mere encreaſe of ſurface upon their being divided into minute parts. Which is wrong. For the ſpecific gravity of any body is only to be altered by making that body occupy a greater or leſs portion of ſpace. Now it is evident that upon dividing a body into parts, the ſurface encreaſes according to the number of parts, but the ſpace occupied, and conſequently the ſpecific gravity, remains the ſame. Take a cube whoſe ſide is ten inches, the ſurface is ſix hundred ſquare inches, the ſpace occupied one thouſand cubic inches; divide that cube into a thouſand cubes; the ſurface will be encreaſed to ſix thouſand ſquare inches, but the ſpace occupied will remain a thouſand cubic inches, and therefore the ſpecific gravity will be the ſame; and ſo on divide it into as many parts as you pleaſe. Indeed bodies, by an encreaſe of ſurface, meet a greater reſiſtance in paſſing through any medium; but I think reſiſtance and ſpecific gravity are here carefully to be diſtinguiſhed. For that reſiſtance which prevents the ſinking of the minute parts of a body, in a fluid ſpecifically lighter, muſt equally retard their aſcent in the ſame fluid; and therefore can never be the cauſe of their aſcending.

But let us ſuppoſe that this veſicle is formed and filled with rarified air, and aſcending through the atmoſphere; it is plain, from another cauſe, that it cannot remain ſo during a few ſeconds of time: for no ſolar or culinary heat can expand air, ſo as to make it per manent in that ſtate, in a veſicle of vapour, aſcending through the open atmoſphere, for the ſpace of one minute; for it is evident, that metallic bodies, heated many degrees hotter than boiling water, and exceedingly larger than the aſcending veſicles, cool in a few ſeconds, when expoſed to the open atmoſphere. For inſtance, mechanic operators, in tempering ſmall drills, only heat them red hot in the flame of a lamp, and then waving them three or four times through the air, find them not only cold, but hardened by their ſudden cooling, ſo as to be fit for cutting other parts of the ſame ſteel, which has been cooled more gradually. Now, the veſicles, being exceedingly ſmaller than the points of ſuch drills, muſt cool much quicker, and the included air be condenſed equal to the circumambient air almoſt inſtantaneouſly; in which ſtate the veſicle is not fitter for aſcent, than that portion of water was before the veſicle was formed; for though the ſurface of the water is vaſtly encreaſed by the form of the veſicle, which may retard the deſcent of it through the air; yet that form muſt equally obſtruct its aſcent; and the ſpecific gravity of the conſtituent particles remaining the ſame, I can't ſee how the encreaſe of ſurface can aid its aſcent. And here I muſt obſerve, that it has not yet been explained how water can be dilated, ſo as to occupy eight hundred or a thouſand times the ſpace which it naturally does. For I think the greateſt expanſion it ſuffers by boiling (the greateſt heat we can give it) is not more than a nineteenth or twentieth part of what it was before.

But, ſuppoſing theſe veſicles formed and aſcending; there is ſtill a very neceſſary power wanting, I mean repulſion, to prevent their running into contact (by their natural attraction) and forming larger maſſes; in which ſtate they would immediately deſcend.

Now let us ſuppoſe that they aſcend by impulſe; it will be very difficult to explain their motions which are evident to ſenſe. For if they aſcend by impulſe, their motion muſt be continually retarded by their gravity and the obſtruction of the air, until they arrive at their greateſt height, which muſt always be in proportion to their firſt velocity; and then they muſt deſcend by the laws of ſpecific gravity, as all other bodies do. The velocity with which we generally ſee vapour aſcend, is not ſufficient to carry it a few yards in height; and therefore, if there was no other cauſe of aſcent but impulſe, the vapours and exhalations muſt be pretty equally diffuſed over the ſurface of the earth, aſcending to a ſmall height, and immediately deſcending again: Or, if it were poſſible for them to float at that height, the conſequence muſt be a perpetual fog. How far the great uſes of viſion would be obſtructed or loſt in this caſe, I need not mention; nor need I ſay how detrimental it would prove to reſpiration, ſince mankind have agreed to call fogs unwholeſome. However, when I come to ſpeak of the uſes of electrical fire in animal life, I ſhall ſay ſomething on this ſubject.

However neceſſary an impulſive power may be in the firſt emiſſion of vapour and exhalation from their maſſes, I think it evident, from the ſlowneſs of their aſcent, that it cannot carry them to that height where clouds are uſually formed; much leſs is it poſſible that it ſhould be the cauſe of their floating there for many days, as we ſee they do. This can only be done by an alteration of their ſpecific gravity, till they are brought into equilibrio with that part of the atmoſphere where they float.

It now remains to enquire by what means this may be done; ſince neither impulſe, rarefaction of the air, or any formation of their parts by expanſion (which I know of) ſeem ſufficient for the purpoſe.

There appears to me but one way of altering the ſpecific gravity of the particles of vapour and exhalation to render them lighter than air; which is by adding to each particle a ſufficient quantity of ſome fluid, whoſe elaſticity and rarity are exceedingly greater than that of the air. That the fluid or fire of electricity is ſuch, I believe will be eaſily granted me; but how far it is adapted to this purpoſe, we muſt enquire from experiments.

But, firſt, let me not be thought abſurd in ſaying that the Creator might have thought proper to adapt this fluid to this particular purpoſe; for the purpoſe is great, no leſs than all vegetation and animal life depending on the aſcent and deſcent of vapour and exhalation.

I need not run into a prolix detail of electrical experiments to prove the properties of this fire, which are much better known to you. Altho' I have made ſome experiments, which, perhaps, have not yet come before you; by which it appears that all fumes ariſing from fire, whether blazing or otherwiſe, and all ſteams riſing from boiling or warm waters, and from all other fluids, and the breath of man and of all other animals, and all the effluvia thrown off by perſpiration, are all ſtrongly electrified; but I ſhall not trouble you with theſe until I come to ſpeak of the uſes of this fire in animal life. I ſhall now only mention a few which are well known, and which are to my purpoſe. Firſt, that deſultory motion by which it flies off from an electrified body to any number of non-electrics which are brought within the ſphere of its activity and affection, until it be equally diffuſed through all. And, Secondly, that the ſphere of its activity is encreaſed by heat. Thirdly, that this fire does not mix with air. Fourthly, that it intimately pervades water, and many other bodies, covering their ſuperficies to a certain diſtance; which diſtance is not in proportion to the bulk of the body electrified, but in proportion to the ſtate of activity of the electrical fluid. Fifthly, this electrical fluid readily joins with any fire which fumes, or rather with the blaze, or fumes of any fire; but will not mix or fly off with the fire of red-hot iron, or any other metal which *does not fume.* I have not met with this obſervation in any writer on electricity, but I have proved it by experiment.

Now to ſhew that this electrical fire or fluid is the principal cauſe of theaſcent of vapour and exhalation, we need only prove that it attends all vapour and exhalation, and that in ſuch quantity as is neceſſary to render them ſpecifically lighter than the lower part of the atmoſphere.

I ſhall not undertake to determine by what cauſe vapour and exhalation are detached from their maſſes; whether by the ſolar or culinary fire, or by the vibrations of the electrical fluid rendered more active by thoſe fires; tho' I am led to think the latter. But it is evident that they are united in exceeding minute diſtinct particles, and that theſe particles muſt paſs through that electrical fluid which ſurrounds the ſurface of the maſs, and that, by that means, they muſt be equally electrified with the maſs; that is, they muſt be covered with the electrical fluid to as great a diſtance from their ſuperficies as the maſs is covered; which muſt always be in proportion to the ſtate of activity of the electrical fluid. In which ſtate, when they have paſſed the ſurrounding fluid, they muſt be repelled by it, and alſo repel each other; and if each particle of vapour and its ſurrounding fluid occupy a greater ſpace than the ſame weight of air, they muſt be fitted to aſcend 'till they come in equilibrio with the upper and rarer part of the atmoſphere, where they muſt float until their ſpecific gravity is altered. As it is very difficult to aſ-ſign the magnitude of each particle of vapour and exhalation, and that of the ſurrounding fluid, and to ſhew that both taken together occupy a greater ſpace, than the ſame weight of air; we can only apply to experiment to ſhew that it is poſſible that it may be ſo; and that will ſhew that in all probability it is ſo; ſince it is evident that every particle muſt be endued with a portion of this electrical fire or fluid; and that there is not any other ſufficient cauſe aſſigned for their aſcending. It is evident that upon electrifying any light matter, ſuch as down or the downy parts of feathers, that their ſpecific gravity is much leſſened, and that by holding another electrified body under them, they may be driven upwards at pleaſure. It is alſo evident from experiment, that the more you divide the parts of ſuch bodies, the more of their ſpecific gravity will they loſe by being electrified; and by dividing them into minute parts, I have found, that they aſcended to a conſiderable height after they were electrified. From whence I think it highly probable that the exceeding ſmall particles of vapour and exhalation may be, and are ſufficiently electrified to render them ſpecifically lighter than the lower air, and that they do aſcend by that means. And that they will aſcend proportionally higher as the ſurrounding fluid is proportionally greater than the particle which is carried up.

It may be now thought neceſſary to ſhew what becomes of our vapour and exhalation *above*, or how they are to come down again.

But I muſt here beg your patience while I endeavour to ſhew that the aſcent and deſcent of vapour and exhalation attended by this fire is the principal cauſe of all our winds. In doing of which I hope to bring down the vapours and exhalations, and explain the general phænomena of the weather and barometer.

It being admitted that wind is only air put into motion, many have been the conjectures how that motion is cauſed. Among which the motion of the earth, and the air's being rarefied by the ſun ſeem to ſtand firſt. The trade winds being moſt regular, and occupying a conſiderable part of the globe, it has been thought proper firſt to account for them from the afore-mentioned cauſes. But I think that theſe cauſes, by themſelves, are not ſufficient for the motion of thoſe winds, and much leſs ſo for the irregular motion of all the other winds. If the apparent motion of the air was occaſioned by the diurnal revolution of the earth from weſt to eaſt, by the air's being left behind, the motion muſt be found more regular, and very different from what it is; for in that caſe the greateſt motion muſt be at the equator, and from thence leſſen by regular degrees to the poles, and muſt be continued always equally one way, both day and night, and at all ſeaſons. But we find quite the contrary, the moſt gentle gales blowing at the equator, and between the tropics, pretty ſteadily one way all day long, and dying away at night; while high winds and ſtorms, blowing all manner of ways, are found in the higher latitudes.

I ſhall not detain you to ſhew that the ſun's rarefying the air cannot ſimply be the cauſe of all the regular and irregular motions which we find in the atmoſphere; but I ſhall proceed to what I think is the cauſe, *viz.* The aſcent and deſcent of vapour and exhalation attended by the electrical fire or fluid.

You are well acquainted with the calculations (by ſundry authors) of the quantity of vapour raiſed in a day; but I will venture to ſay, that all thoſe I have met with fall exceedingly ſhort of the quantity raiſed in the torrid zone. Now all this vapour and exhalation being buoyed up by the electrical fire, muſt add a column to the air, tho' of a different matter, at leaſt a thouſand times greater than the vapour and exhalation taken up; which column muſt neceſſarily force the adjacent part of the incumbent air upwards; and muſt as neceſſarily be re-acted upon by the incumbent air, to reſtore the equilibrium of the whole air. And as it cannot be readily forced down again, it muſt float off, at that altitude, toward thoſe parts where little or no addition has been made to the atmoſphere, and by that means muſt propel the air on the horizontal level with it, and that below it as it is itſelf propelled by the weight of the incumbent air; and that motion muſt be from the equator, where the greateſt quantity of vapour, &c. is raiſed, toward the poles, and partly to the weſt, as the earth turns toward the ſun. For here we muſt confeſs that the ſun is the great agent in detaching vapour and exhalation from their maſſes; whether he acts immediately by himſelf or by his rendering the electric fire more active in its vibrations. But their ſubſequent aſcent I attribute entirely to their being rendered ſpecifically lighter than the lower air by their conjunction with this electrical fire.

The fire which ſurrounds the vapour beginning to condenſe, and the vapours to ſubſide in paſſing the tropics becomes a greater preſſure on the air beneath, and by that means forces ſome part back into the tropics in the place of that air protruded by the aſcent of the vapour, &c. and the remainder in a direction toward the poles. The common rotation of the air in coming in below to ſupply the place of that part carried up by any fire may explain this motion.

To ſhew how this motion muſt tend to the weſt, we muſt conſider that the column of air, raiſed by the aſcending vapour, &c. is at its greateſt altitude to the eaſt; and therefore muſt preſs that air to the weſtward which is continually protruded by the vapours, &c. beginning to aſcend from eaſt to weſt; and the compreſſed air at the tropics muſt tend to the weſtward, 'till their forces incenting make the motion intirely to the weſt. The air itſelf being rarefied and carried up by the reflection of the intenſe heat of the ſun (for heat in flying off from all bodies does aſcend through the air with great velocity) may be a conſiderable additional cauſe of theſe trade winds; but never can be the ſole cauſe of all the erratic winds.

As I am not ſufficiently informed of the particular formation of the globe, to account for all the irregular winds within the tropics, I ſhall only venture to ſay that, where ſuch happen, it muſt be by means of ſome tracts of lands which riſe to a greater height above the horizontal level, than vapours generally do; by which the motion of the vapour is ſtopped, and the vapour accumulated by ſucceeding vapour, and the air upon which they float is of conſequence preſſed into a new direction.

And from hence may alſo be explained the cauſe of the rains, particularly ſo called in the ſea language.

I muſt now conſider what becomes of the vapour, &c. floating from over the tropics toward the poles, which being leſs affected by the heat of the ſun, reflected from the ſurface of the globe, the ſurrounding electrical fire begins to condenſe more and more as it moves toward the poles, and the vapour of courſe to deſcend, and that part moſt, which is moſt remote from, or is fartheſt left behind by the ſun; and of conſequence the higher column of air muſt tend that way to reſtore the equilibrium, which motion, at this ſide the equator, muſt be to the north-eaſt; and as the vapour, &c. fall again to the earth the motion muſt be more to the eaſt. From whence our ſouth-weſt and weſterly winds, which blow a conſiderable part of the year. But as this ſyſtem is too regular to account for the phænomena of the erratic winds, we muſt conſider whence they ariſe. I have before obſerved that tracts of land riſing into the atmoſphere will ſtop the regular motion of the vapour, &c. and that the vapour being accumulated by ſucceeding vapour, the ſubjacent air muſt be preſſed into new directions. Now this cauſe added to the daily dilatation of the electrical fire, and the contraction at night, and the coaliſion of vapours to occaſion their total deſcent, will be ſufficient to produce a very great variety of winds on this ſide the tropic. But if I were to ſet down all that has occurred upon this ſubject about upper currents, under currents, and intermediate tranſverſe currents, &c. occaſioned by the various motions of the atmoſphere to reſtore an equilibrium; and, to produce my proofs, I muſt ſend you a book inſtead of a letter; but I write not to common minds.

It now remains to ſhew how the general phænomena of the weather and barometer ariſe from this ſyſtem. Firſt, why it generally rains in the winter, while the wind is ſouth, ſouth-weſt, and weſterly. Secondly, why north-weſt winds are generally attended by ſhowers in the beginning, and become more dry, as they are of longer continuance. Thirdly, why north and north-eaſt winds are generally dry. Fourthly, why the eaſt wind continues dry and dark for a conſiderable time together. Fifthly, why ſqualls precede heavy and diſtinct ſhowers, and a calm enſues for ſome little time after they are paſt. Sixthly, why ſtorms and high winds ſeldom happen in a ſerene ſky without clouds. Seventhly, why the vapours in warm ſeaſons coaleſce to form thoſe diſtinct, denſe clouds, which produce thunder and heavy ſhowers. Eighthly, why the barometer falls loweſt in long continued rains attended by winds, and why it riſes higheſt in long continued fair weather; and why the intermediate changes happen. Ninthly, of land and ſea breezes, and water-ſpouts.

Firſt, the vapours, paſſing the tropics into the colder regions, have their ſurrounding fire condenſed by degrees, which muſt encreaſe their ſpecific gravity, and leſſen their repulſive power; by which means they muſt both deſcend and approach each other, 'till at laſt they form denſe, viſible clouds; and theſe clouds are alſo accumulated by other ſucceeding vapours, of like ſpecific gravity, 'till they form clouds, which are often ſeveral hundred yards in depth; which I have often ſeen, in paſſing through them up the ſides of very high mountains. In clouds of ſuch depth, I think the coaliſion of their particles, to form drops, may ariſe from their motion, and the order of ſpecific gravity. For, the air being a great deal denſer, and of greater ſpecific gravity, at the bottom of ſuch a cloud, than it is at the top; and the particles of vapour, with their fire, being pretty near of an equal ſpecific gravity;

thoſe particles which are below muſt neceſſarily be forced upwards, and thoſe above muſt as neceſſarily deſcend, 'till they all meet at that altitude, whereat their ſpecific gravity would make them float; if they were not prevented merely by their repulſive power; but this repulſive power muſt be greatly acted upon, if not quite overcome, in ſome places by this preſſure of the air and of the vapours. But ſuppoſing the particles only brought into a very near approach by this preſſure, it will be ſcarcely poſſible for them, upon any motion of the clouds, to keep their ſtated diſtances, without impinging upon one another: And, whenever this happens, it is eaſy to ſhew how rain is formed. For, wherever two or more particles are forced to approach within their repulſive powers, they will run into contact and ſphericity, (if fluid) by their attractive power, and by that means become covered with the electrical fluid to a greater diſtance from their ſuperficies, than the other particles are, or than they were before their conjunction; (as I have ſhewn in my letter concerning the cauſe of thunder) that is, they will be more electrified; and conſequently, as appear by electrical experiments, the exceſs of electrical fluid will run off among the other particles; by which means the enlarged particles have their ſpecific gravity encreaſed, and are enabled to deſcend to a lower region of the air; and the more particles they impinge upon in their deſcent, the more will their ſpecific gravity and velocity be encreaſed; and the more their velocity is encreaſed, the more particles will they imping upon, till they fall from the clouds in drops, whoſe bigneſs will be according to the depth and denſity of the cloud they have paſſed through.

It may be neceſſary to ſhew, why I aſſert that the encreaſe of velocity in the enlarged particle will make it imping upon a greater number of the ſmaller particles, in its paſſage through them. And this will appear by electrifying a feather, which may be driven upwards, by the electric body, with a certain velocity; but if you add a much greater velocity to the electric body, it will overtake the feather, and get within their repulſive power, and the feather will ſtick to it by their attractive powers. And the leſs diſtance the feather keeps above the electric body, the leſs encreaſe of velocity is neceſſary to make them join. You'll pardon my prolixity in this particular; becauſe I think much depends upon it, in the formation of rain.

Having ſhewn how I think vapours may coaleſce into rain, I ſhall now endeavour to explain the phænomena of the weather, &c.

And firſt, why our ſouth, ſouth-weſt, and weſterly winds are wet in winter, I have before ſhewn that the wind, and vapour with it, muſt tend from the tropic toward the pole, and how it may be varied to the north-eaſt, and to the eaſt. In the cold winter ſeaſon, the vapour, in this courſe, muſt coaleſce more and more as it comes into the colder regions, and at laſt come down in miſts and rain; and the more the column of air is decreaſed by the fall of ſuch miſts and rain, the more room is there for ſucceeding vapour to be preſſed into the ſame courſe, and to fall in like manner, and ſo on, which may occaſion the continued courſe of theſe winds and wet, which generally attends our winters. But ſometimes at the fall of theſe rains, the wind comes in from the north-weſt, to reſtore the balance of the atmoſphere, and rolls the vapour, ſtill floating in the air, (by altering its motion) into heavy denſe clouds, which, upon their ſudden coalition, let fall heavy ſhowers; and by that means the remaining particles of vapour in the cloud have a greater quantity of electric fluid diſtributed among them, which enables them to aſcend, and form what the ſailors call an hard dry ſky; and as they approach farther into a warmer climate, which encreaſes the power of the electrical fluid, they riſe, diſſipate and vaniſh out of ſight; and this is the general conſequence of north-weſt winds. As to the north and north-eaſt winds, whatever vapour they bring with them, has its repulſive and aſcending power (that is the electrical fluid) ſo encreaſed by approaching into warmer and warmer climates, that it ſcarce ever comes down in rain, except from the effects of ſome upper current of the air.

It ſometimes happens, while the wind blows very gently at eaſt, that vapours coaleſce at a conſiderable height in the atmoſphere, ſo as to form one even, uniform cloud, ſufficient to ſhut out the bright ſhining light of the ſun, which is therefore called a dark ſky; which cloud being uniform, and of no great thickneſs, and carried on by a very gentle motion, and that neither approaching a warmer or a colder climate, and ſeldom deſcending ſo low as to be diſturbed by the tops of mountains, the vapours may keep the order of their ſpecific gravity for a conſiderable time, as they generally do; till ſome croſs or oppoſite wind forces them to ſeparate into denſer clouds, and leave apertures for the ſun to ſhine through; and this is generally the caſe for a day or two before any wet deſcends.

Why ſqualls precede heavy diſtinct ſhowers, and a calm enſues for ſome little time after they are paſt, is pretty evident; for the deſcending rain, attended by the electrical fluid, forces the ſubjacent air out of its place, and that which is driven forward muſt add a proportional velocity to the motion of the wind that way, and that which is driven backward muſt alſo obſtruct the motion of the wind advancing after the cloud. The wind preceding the cloud is alſo preſſed off obliquely toward either extreme of the ſhower. Which may be of good uſe for ſailors to know and obſerve; for if they ſail upon a wind from the center of the ſhower toward the extremity, they may ſafely venture to keep their luff; but if they ſail from the extremity toward the center, if they luff up, they will be taken aback, and run the hazard of being diſmaſted; and this I always found true in practice.

Why high winds are ſeldom found in a ſerene ſky without clouds is alſo pretty evident; for clouds occaſion theſe high winds in a double manner; firſt, when they are large and united and upon the deſcent, as they occupy a great ſpace in the atmoſphere, they muſt preſs the ſubjacent air into a great velocity; and ſecondly, when they coaleſce and come down in heavy rains, they make room for the air to flow in with violence to reſtore the equilibrium of the whole air.

To know how the vapours coaleſce in warm ſeaſons to form thoſe diſtinct denſe clouds, which produce thunder and heavy ſhowers, we muſt firſt conſider the ſtate of the vapour in its aſcent, which muſt be ſtrongly endued with the electrical fire to enable it to aſcend to the great height, it then does, in the atmoſphere; ſecondly, the great quantity carried up and kept afloat at that great height during a ſeries of hot dry weather. The quantity may be proved from the height of the mercury in the barometer (as I ſhall ſhew preſently) and the great height and diſſipation of the vapour from the ſerene appearance of the ſky; and alſo by the great height where even thunder clouds are formed in its deſcent; which may eaſily be meaſured by the length of time between the light and noiſe of thunder. In this ſtate the upper air is in a quietude, and whatever part of this vapour begins to coaleſce and ſubſide firſt, will carry down with it part of the ſubjacent vapour, and make room at the top for other vapours to flow in from all ſides, which, by their meeting, may alſo coaleſce and ſubſide to form this central deſcending cloud; and ſo alſo of like ſucceeding vapours. And thus I think clouds of any denſity may be formed, in a quiet air (and thunder generally happens in calm weather) 'till the denſity is too great for the buoyant electrical fire to ſuſtain the weight; which flying off in part from the largeſt drops, lets them fall almoſt in ſpouts.

Why the mercury falls and riſes in the barometer will appear from what I have been ſaying; for in long continued fair dry weather there is a great quantity of vapour raiſed and kept afloat in the air; which muſt neceſſarily add a column to the air at leaſt a thouſand times greater than the vapour taken up, and oftentimes much greater than that, which muſt as neceſſarily encreaſe the weight of the air, and make the mercury riſe proportional to the additional column; and when rains deſcend, they ſubtract a column from the air near a thouſand times as great as the water fallen, which muſt neceſſarily leſſen the weight of the air, and make the mercury fall proportional to ſuch ſubtraction.

As to winds, they affect not the mercury, but as they are generally, in theſe climates, a conſequence of deſcending clouds and rains, blowing into thoſe parts where the column of air has been leſſened, and while they blow, the mercury will continue low in the barometer, 'till the equilibrium of the atmoſphere be reſtored. But I believe if it were enquired into, whether the mercury deſcends when the wind begins to blow every morning between the tropics and the vapour to aſcend, that it will be found that the mercury rather aſcends.

There is a phænomenon which has puzzled many, that is why the mercury falls before the rain falls. This may be explained by the column of air's being leſſened gradually, by the condenſation of the electrical fluid from the firſt deſcent of the vapours till they fall in rain.

I ſhall now ſay ſomething of land breezes and ſea breezes, a phænomenon which ſometimes happens in fair ſettled weather; that the wind blows out from the land at night, and in from the ſea at day-time. The land breeze is occaſioned by the deſcent of clouds and the particular formation of the land; for if the land riſe into an hilly country from the ſea, when the clouds and vapours deſcend at night, which they often do by the electrical fluids being condenſed, they muſt preſs the air down the land toward the ſea in their fall; as may appear from the ſmoke of any fire running down the ſide of an hill, in the evening of a damp day when the clouds are upon the deſcent. And the ſea breeze is occaſioned by the clouds aſcending in the day-time, which muſt impel the incumbent air upwards, and make room for the ſea breeze to flow in; but beſide the mere aſcent of clouds there is an exceeding greater quantity of vapours raiſed from the land than from the ſea. For the ſame extent of land has an exceeding greater ſurface than the ſame extent of ſea; which may appear from the various forms of vegetables and animals, &c. and the greater the ſurface the greater will the evaporation be; beſide the more irregular theſe ſurfaces are, the greater will be their reflection and refraction of the ſun's beams, which will encreaſe their power. And it is alſo neceſſary that the evaporation ſhould be much greater from vegetable and animal fluids, than from fluids in a quieſcent ſtate, to carry on a circulation for the great work of nutrition. But of this when I come to ſpeak of vegetation and animal life. Now the aſcent of theſe vapours muſt beget a circulation of the air inward from the ſea; in the ſame manner as the aſcent of vapours from any fire brings in the air below to that fire.

As to water ſpouts I have found them oddly deſcribed by the learned; as being great columns of water ſucked up from the ſea by the clouds. But I never ſaw any ſuch; nor could I find upon enquiry from many honeſt men, who have ſailed almoſt all our known ſeas, that they ever met any ſuch; and therefore I do not believe that there are any ſuch. There is indeed an appearance ſomething like their deſcription, which may have given riſe to their conjectures; but this is no more than a very heavy ſhower from a very denſe cloud, which is drawn into a conical form, and a very narrow compaſs at bottom, before it arrives at the ſea; which it daſhes with great violence in its fall. It may be worth the while to enquire how the ſhower comes into this conic form; which I think is cauſed by the general attraction. For while the vapours floated in the air, being of like ſpecific gravity, they were equally attracted by the air as by one another; but when their ſpecific gravity was greatly encreaſed by their being formed into larger drops, and letting go great part of that elaſtic fluid which buoyed them up, and the greateſt quantity of water falling in the center of the ſhower, the extremes may be greatly attracted to the center, in their paſſage downwards.

There is one objection may be made to all I have been ſaying of the winds and weather; that is the great diſtance of the tropic, and that the wind muſt be a great time in coming to us from thence. But this objection will leſſen, when the velocity of the wind is conſidered, which in a freſh gale will move above a degree in two hours; which will ſoon bring it from the tropic to us. And this velocity, and a much greater, I proved by a machine, I invented and made in the year 1733, to go upon wheels by the force of the wind. For I have been carried in it, at the rate of more than a degree in two hours, when the wind was upon the beam, that is when I could ſail forwards or backwards upon the ſame tract with equal facility; ſo that my motion, in going before that wind, muſt have been much greater; which I then neglected to eſtimate.

And now, gentlemen, I fear two things, that I have ſaid too much, and that I have ſaid too little; for if this letter appears with the ſame face of truth to you as it does to me, I have omitted many things, for fear of being too prolix, which I ought to have added; but if I have written without a foundation in truth; I muſt, long before you come to this part of my letter, appear prolix and impertinent; and therefore it may be time to conclude; before I do which, I will aſſert one thing, that is, if any thing in this letter is found worth your acceptance, that it is intirely my own; for I have not borrowed a ſingle hint; no more I did in my letter concerning the cauſe of thunder; for when I wrote that letter, which was on the 20th *Sep.* 1751, I did not know that any one had made the leaſt conjecture that the electrical fire was the cauſe of thunder. And I had told many gentlemen in this kingdom, in the beginning of the year 1751, that I thought I had diſcovered the real cauſe of thunder. Indeed immediately after my ſending that letter to you on the 18th of *June*, 1752, I met with ſome accounts in our news-papers that iron bars, ſome how or other, ſet up during the time of thunder, were found to be electrified; but from that time to this I know nothing particular of thoſe or any other experiments on thunder clouds. I waited the publiſhing of the volume of Philoſ. Tranſactions for the year 1752, to inform myſelf of theſe matters; but I don't find that it is yet come to our kingdom. As I am not a plagiary, ſo I would not willingly be *thought* one; nor ſhould I venture to trouble you with what I thought you may have better from other hands; but as the matter of this letter appears new to me, ſo I venture to lay it before you, without any other motive than to add my mite in the inveſtigating truth.

I am,

GENTLEMEN,

Your moſt humble and

moſt obedient ſervant,

HEN. EELES.

My fears of treſpaſſing upon your patience had almoſt made me forget to explain how far the wind aſſiſts vapours to riſe in greater quantities and in leſs time than they would in a quiet air. And this omiſſion would not be excuſable, becauſe both the learned and unlearned have obſerved that a greater quantity of vapour is carried off in windy weather, than when the weather is calm. When vapours are united in great plenty from the ſurfaces of fluids, ſuch as from a pot before it begins to boil, or from the ſurface of a bowl of hot punch, &c. it often happens that they crowd each other in ſuch manner that the ſurrounding electrical fluid of each particle is compreſſed, and cannot occupy that portion of ſpace which is neceſſary to make it buoy up the particle; and thus the particles float in a ſteam juſt above the ſurface of the liquor, and they are prevented from falling back into the liquor by the electrical fluid on the ſurface of the liquor. In which ſtate, none but the uppermoſt particles can aſcend; which they do by the elaſticity of the electrical fluid forcing them upwards, till they have room for the electrical fluid thoroughly to expand itſelf, ſo as to be able to buoy up the encloſed particle of vapour. In this ſtate the aſſiſtance of the wind will be eaſily underſtood; for the wind blowing them from the ſurface, diſſipates them and gives them room to aſcend, and clears the ſurface to make room for the ſucceeding vapours to aſcend, and ſo on; which in this caſe will greatly aid the aſcent of vapour. And thus far I think the wind aſſiſts in the aſcent of vapour, but no farther. I have mentioned hot liquors only, becauſe the ſteam is more viſible in them, not but the ſame caſe often happens in evaporation, where we cannot ſo eaſily perceive it.

To the Preſident, Council,
and Fellows of the Royal Society.

⁂ Sent this letter, dated the 10th of *November*, 1754.

The ANSWER to the Second Letter.

SIR, *London, March* 6th, 1755.

I HAVE deferred my acknowledgments for your letter, and two packets directed to me, 'till I had read your papers addreſſed to the Royal Society, who return you their thanks for them. It would be a further obligation on them, if you would favour them with the experiments made by yourſelf, to which you refer for the ſupport of your reaſonings.

Your communications will always be highly acceptable to them; as any of your commands will be to,

SIR,
Your moſt obedient, and
moſt humble ſervant,

To Henry Eeles, eſq;
at *Liſmore.*

THOMAS BIRCH,
Secret. R. S.

N. B. No anſwer to, or any notice taken of, any of my other letters; tho' repeatedly required by me.

HENRY EELES.

LET-

LETTER IV.

SIR, *Liſmore*, 21ſt *March*, 1755.

I RECEIVED your obliging favour of the 6th Inſt. of which I am highly ſenſible; and ſhall always be ſubſervient to your commands.

You do me the honour, to tell me, that the Royal Society deſire to know the experiments, by which I found all aſcending vapours and exhalations to be electrified. At firſt I only ſuppoſed that they muſt be ſo, according to the reaſoning in my letter; but, upon trial, with a very ſimple apparatus, I had the pleaſure to convince myſelf that they were ſo.

I extended a fine ſtring of ſilk eight feet horizontally; and from the middle ſuſpended two pieces of ſuch down as grows upon our turf-bogs, by two pieces of fine ſilk, about twelve inches each in length; and then, by rubbing a piece of ſealing-wax on my waiſtcoat over my ſide, I electrified the pieces of down; and then brought ſundry burning things under them, ſo as to let the ſmoke paſs in great plenty through and about them, to try whether the electrical fluid would run off with the ſmoke; but had the pleaſure to ſee that the down was but a little affected by the paſſage of the ſmoke, and ſtill remained electrified. I then brought ſundry ſteams from the ſpout of a boiling teakettle, and otherwiſe in the ſame manner; and ſtill found the down remained electrified. I then breathed on them in great plenty, but found that the down ſtill remained electrified. I then joined the palms of my hands together, with the fingers extended perpendicularly under the down, which ſtill remained electrified; although the ſubtile effluvia, thrown off by perſpiration, paſſed in great plenty through the down; as may appear by holding one or both the hands in the ſame manner under any light matter floating in the air, which will be driven upwards thereby, with as great velocity, as an electrified feather is by an electrified body held under it. In ſhort, I tried all the vapour and exhalation I could think of, in the ſame manner, and with the ſame ſucceſs.

I then warmed a wine glaſs, and, with the ſkirt of my coat, held inſide and outſide the glaſs between my fingers and my thumb, I rubbed the glaſs briſkly about, and electrified the down, and found all experiments anſwer in the ſame manner as they did with the wax. I mention this particular, becauſe ſome writers on electricity have ſaid that there were two kinds of electrical fire, the one reſinous, and the other vitreous; becauſe light bodies electrified by glaſs, are attracted by electrified wax, &c. and thoſe electrified by reſins, are attracted by glaſs. But I think theſe different effects muſt ariſe from ſome differing qualities in the reſin and glaſs, which have power to actuate this fire differently. For if there were really two diſtinct ſpecies of this fire oppoſite in their nature, the afore-mentioned experiments would have a very different conſequence from what appears. For if the vapours were impregnated by the vitreous fire, they muſt abſorb, or ſome way diſturb the reſinous fire which electrifies the down; and ſo, *vice verſa*; but we find that the ſame vapour, with its electric fire, paſſes through the electrified down in the ſame manner, whether it be electrified by glaſs, or reſin. But I will not detain you on this ſubject.

The electricity remaining in the electrified down after theſe experiments, made it appear that the ſmoke and ſteams muſt be either electrics, or non-electrics electrified. It was eaſy to ſuppoſe them non-electrics; as they ariſe from non-electric bodies; and the more, becauſe the higheſt electrics, by a diſcontinuity and comminution of their parts (long before they come to be as minute as the particles of aſcending vapours) become non-electrics, or conductors of electricity. For glaſs, reſin, wax, &c. all become non-electrics, even in fuſion.

But, to try whether the ſteams, &c. were non-electrics, I only bedewed the wax and glaſs with my breath, ſteams, &c. from my hand to the end of the wax and glaſs; and then touching the electrified down with the ends of the wax and glaſs, I found that the electrical fire immediately paſſed from the down into my hand through the ſteams, &c. which reſted upon the wax and glaſs; which I think ſufficiently proves the ſteams, &c. to be non-electric; and I think it as plainly appears, that they are electrified while aſcending, becauſe the electrical fire in the down, does not join with them in their paſſage through it; which otherwiſe it would do with them or any non-electric not electrified.

I made ſome other experiments to this purpoſe, but theſe principally perſuaded me to ſay that all aſcending vapours and exhalations were electrified; but how far they will weigh with the Royal Society, I muſt leave to their better judgment; and am,

SIR,

Your moſt humble, and
moſt obedient ſervant,

HEN. EELES.

To the Rev. Mr.
Th. Birch, Secretary to the Royal
Society, in Crane-court, Fleet-ſtreet,
London.

I electrified the down after each experiment.

LET-

LETTER V.

SIR, *Liſmore*, 12th *April*, 1756.

I Wrote to you 21ſt *March*, 1755, with ſome electrical experiments, and therein mentioned, that ſome writers were of opinion that there were two kinds of electrical fire, the one reſinous, and the other vitreous; becauſe light bodies electrified by glaſs are attracted by the electrified wax, &c. and thoſe electrified by reſins are attracted by glaſs. But that I thought theſe different effects muſt ariſe from ſome differing qualities in the reſin and glaſs, which have power to actuate this fire differently. I have often ſince been concerned that I did not communicate an experiment that I had then, and many times before made, the effects of which, and many others I have ſince made, ſome few of which I ſhall mention, may let us farther into this enquiry, and give us a new doctrine of electricity.

It may appear ſomething late to trouble you on this ſubject after the labours of ſo many ingenious men; but as I think that there are but few things which can open a larger field for ſpeculation to the inquiſitive, than the powers of electricity in the operations of nature; I will venture to lay before you ſome diſcoveries which may be uſeful in inveſtigating the powers and uſes of this active principle.

I ſhall alſo boldly venture upon another beaten track, I mean magnetiſm, where I think there is ſtill room for enquiry, tho' numberleſs men, of abilities ſuperior to mine, have gone before me; but as truth is what we all ſeek, I ſhall not be aſhamed to lay any diſcoveries before you which appear new to me; in which if you find nothing uſeful, I hope you will forgive the trouble of reading, and diſpoſe of them as you think proper. But I will add my opinion, that I think it one great obſtruction in the way to knowledge, that from ſomething deſultory in the mind, men generally act like children in growing too ſoon tired of the ſame play-thing, and always quit theſe difficult reſearches, long before they come to the extent of the human underſtanding.

There are in electricity two different diſtinct powers, which in their natural ſtate are always conjoined, and ſeem to exert but little ſenſible action in that ſtate; theſe powers I call, only for diſtinction ſake, the vitreous power and the reſinous power, becauſe they ſeem to be differently excited by wax and glaſs; not that one is excited without the other, for they are both always excited together, the one acting inward to the electric and the other out from it, as often as they are forced to ſeparate by heat and friction; and this order is tranſpoſed as often as the poliſhed glaſs and wax are rubbed; that power which acts out from the glaſs acts inward to the wax, and that power which acts out from the wax acts inward to the glaſs. They may be alſo tranſpoſed as often as you pleaſe by rubbing the ſame piece of glaſs, as I ſhall ſhew by experiment. Or you may electrify the ſame matter with which power you chuſe, without tranſpoſing the powers, while they are excited by the wax or glaſs, only by drawing off that power which acts inward to, or outward from the electric. The method of doing which I ſhall ſhew preſently by experiment.

Any two things electrified by any one of theſe powers repel each other; but if you electrify two things, one with the one power, and the other with the other power, then theſe two things will attract each other; which ſhews that theſe powers naturally attract each other, and are only ſeparated by force. This matter being known, it will be much eaſier to account for the phænomena of electrical experiments than it has been hitherto.

I could ſend you a number of complicated experiments to prove this doctrine, and to explain the phænomena of electricity; but I rather chuſe to let it go with as plain and evident experiments as may be, leſt I ſhould err in my manner of expreſſing them, or the operators miſtake in the executing of them, and by that means think me miſtaken in what I aſſert; as I well know how difficult it is, to make gentlemen recede from the opinions they have held and aſſerted, even by the cleareſt truth.

Before I mention any electrical experiments I will ſay ſomething of magnetiſm; as there are ſome things in the powers of magnetiſm mighty ſimilar to thoſe of electricity; but I do not by any means think them the ſame.

Magnetiſm conſiſts of two different diſtinct powers, which in their natural ſtate are conjoined and exert but very little ſenſible action, and ſtrongly attract each other at all times; but when ſeparated by force, they act ſimilar to thoſe of electricity; for when magnetiſm is excited in two different pieces of iron by the ſouth pole, thoſe ends repel each other, the ſame thing follows when they are excited by the north pole; (for ſo theſe different powers have been called from their verticity;) but if one piece be excited by the north pole, and another by the ſouth pole, then they attract each other, as in electricity.

Both magnetiſm and electricity act ſtrongeſt at corners, edges and points, they have their repulſive and attractive powers at certain diſtances; the electrified body is as much attracted by the body which is not electrified, as the latter is by the former; the magnet is alſo as much attracted by the iron, as the iron is attracted by the magnet. But here I muſt remark that the magnet attracts and is attracted according to the quantity of iron to be attracted, and not according to the ſtrength of the magnet. For one ounce of lead at the end of a ſmall bit of iron, will pull it off from a magnet, which will ſuſpend a pound of lead at the end of a piece of iron weighing four or five ounces; tho' the ends of both pieces of iron, which are to come in contact with the magnet, be ſhaped alike; the cauſe of which I ſhall endeavour to explain. This has ſomething ſimilar to the action of gravity.

Both powers of magnetiſm and electricity are excited and ſeparated by friction; which is a very wonderful effect if duly conſidered in both; particularly in magnetiſm, that theſe powers naturally attracting each other ſhould remain ſeparate in the ſame ſteel bar for many years; and yet at any time be drawn back into their natural ſtate by a few rubs of two other magnets in a contrary order to that which ſeparated them at firſt; I mean by drawing their poles from the center of the bar over the ends, the north pole over the north pole, and the ſouth pole over the ſouth pole, by which means the ſeparated powers are conjoined, and ſeem to vaniſh, at leaſt to be quieſcent; not that they really are ſo, for they are always ſet in action by a foreign power according to the poſition they lie in; as I ſhall ſhew preſently. The powers of electricity will alſo remain ſeparate on a rubbed glaſs, for many hours in a dry air and warm place.

There is another remarkable ſimilarity between theſe powers, for I can give a verticity to a fine needle by the electric powers, and make its poles what points of the compaſs I pleaſe to make choice of; but this verticity is very weak and eaſily altered, and at beſt not permanent for many hours; and therefore I will not trouble you with the manner of giving it. Mr. *Franklin* ſays in his letter, in the 47th vol. of Philoſ. Tranſactions, Number 44, page 289, that he has frequently given a polarity to needles, and reverſed it at pleaſure by an electric ſhock. Which I believe is true; but he does not ſeem to know that the electric fire, in this caſe, does no more than any common fire will do; as I ſhall ſhew preſently.

Magnetiſm is a quality inherent in all iron, nor do I think that iron can be diveſted of it. Fire, which is the readieſt means to diveſt iron of any quantity of fixed magnetiſm, does not diveſt it of its natural quantity, but on the contrary will fix a polarity in any iron, according to the manner of heating or cooling that iron.

Mr. *Robert Boyle* long ſince told the world that iron, heated red hot and cooled in a vertical poſition, acquired a fixed polarity, the under end being the north, and the upper end the ſouth pole. He alſo told them that a bar of iron held vertically, had a polarity in that poſition, in the manner above, but that the poles were reverſed upon reverſing the bar. He has alſo told them how to change the poles of a magnet by fire, and likewiſe how to diveſt a bar of ſteel of its fixed magnetiſm, by a contrary touching; which Mr. *Canton* ſeems to make a ſecret.

Tho' he and others, have told us many things too prolix to repeat, yet I think ſomething may be added to ſhew by what fixed rule magnetiſm is acted on by fire, as alſo to ſhew how it acts in its natural ſtate; and therefore I ſhall add this rule; That all bars of iron, which have not a fixed or unchangeable polarity, acquire their ſtrongeſt polarity while held parallel to the dipping needle; and that polarity leſſens by degrees, as they recline from that poſition, (whether in or out of the magnetic meridian) 'till they come to be perpendicular to the dipping needle; in which poſition the magnetiſm does not ſeem to act. And this may be proved, by touching a fine ſewing needle with the magnet, and floating it upon a glaſs full of very clean water: for, upon bringing an iron bar in the above poſition, the north end of the needle will be repelled by the under end of the bar, and attracted by the upper end of the bar; and ſo, *vice verſa*, of the ſouth end of the needle. And, as you recline the iron bar from being parallel to the dipping needle, you'll find the polarity leſſen by degrees, 'till it becomes perpendicular to the dipping needle; and then all ſigns of polarity vaniſh. What I have now ſaid, will alſo ſerve for a rule for fixing a polarity in bars of iron, or ſteel, by fire; for if they are heated red hot, and cooled in any of the above poſitions, they will have their polarity fixed according to the poſition; the ſtrongeſt, while parallel to the dipping needle, and ſo leſſen by degrees, 'till their poſition becomes perpendicular to the dipping needle, and then they will not have any fixed polarity. And this rule holds invariable, provided that the iron and ſteel be equally heated, and equally cooled, whether in the air or in water. But I have found, upon cooling in water, that when the under end of the bar was exceedingly hotter than the upper, and that the upper end cooled firſt, that it ſometimes became the north pole; but this very ſeldom happened. After heating bars of ſteel very hot, and plunging them into cold water in a poſition parallel to the dipping needle, I have found that they had not only acquired a ſtrong fixed verticity, but alſo an attractive power.

I ſhall now ſay ſomething of communicating this power. Firſt, that large ſurfaces eaſily communicate it to ſmall ones (but not *vice verſa)* and that in a much greater degree than any they exert themſelves: For, take a large bar of iron, or ſteel, which is better, which has not a fixed verticity, and hold it parallel to the dipping needle; then take ſmall bars of ſteel, about three inches long, a quarter broad, and one ſixteenth thick, and touch them to the ends of the large bar, as you would to any magnet, by drawing them from the middle of the ſmall bar over the ends both of the ſmall and large bar, ten or a dozen times with each ſide of the ſmall bar, and you'll find the ſmall bar endued with a ſtrong fixed verticity and attractive power, much greater than what the large bar exerts in any poſition; and changed into a real magnet. And thus, by touching a number of the ſmall bars, and proceeding in Mr. *Canton*'s method, the magnetic virtue may be raiſed to as great a degree as the metal can receive. For here I muſt obſerve that a piece of ſteel well hardened may be made to take up thirty times the weight that it will do when quite ſoft, tho' you touch it with the ſame magnetic bars in each ſtate. And this method comes nearer to what Mr. *Canton* propoſed, *viz.* to make artificial magnets without the help of natural ones. But neither will do that; for his poker and tongs are natural magnets and have their verticity fixed by being heated and then cooled in a vertical poſition; and the iron or ſteel bar, tho' without a fixt verticity, yet while it remains in that poſition exerts a verticity, and is able to communicate a fixed verticity to the ſmall bar, and is therefore for the time a natural magnet. Nor is there any iron bar, from the largeſt to a ſix-penny nail, but what will exert this power according to the afore-mentioned rules; nor can they be diveſted of it.. But how this power is raiſed to degrees exceedingly above that which gave it, and communicated to any quantity of ſteel we pleaſe in the ſame or greater degrees, has been matter of wonder and ſurpriſe; nor has it been more eaſy to account for its being ſo quickly withdrawn from a magnetic bar, by a friction contrary to that which gave it.

But I ſhall now endeavour to explain this matter. The magnetic powers in the large ſteel bar, while it is held parallel to the dipping needle, are acted upon by that foreign power which directs all magnetiſm, one of them being attracted to it, and the other repelled from it, as may appear by the bars exerting a verticity in this poſition; and if the bar continues for a great length of time in this poſition, this action produces a fixed verticity in the bar by ſeparating its powers. And while theſe powers are any way ſeparated in the large bar, if a very ſmall bar, be drawn over the end of the large bar, then that power predominating at the end of the large bar, attracts one of the powers of the ſmall bar to that end, which was in conjunction with the large bar, and conſequently forces the other power to the other end of the ſmall bar, and by a frequent repetition of this operation the powers in the ſmall bar are intirely ſeparated, and a fixed verticity produced. Now when a number of theſe ſmall bars have the powers thus ſeparated, they may be joined in Mr. *Canton's* method to ſeparate the powers in a larger bar, whoſe ſurface ſhould not exceed the ſurfaces of all the ſmall bars taken together; and thus you may proceed to ſeparate the powers in what quantity of ſteel you pleaſe. How they may be again conjoined in their natural ſtate, I have before ſhewn. Why the large piece of iron is more attracted by a magnet than the ſmall piece, appears from there being a much greater quantity of this power to be attracted in the large piece than in the ſmall piece.

I could produce many experiments to ſhew that theſe powers are always ſeparated in any piece of iron while in contact with the magnet, and that they remain ſo for ſome little time after the magnet is withdrawn, which duration is in proportion to the ſize and ſtrength of the magnet and the quantity of iron; but this would make me run into an unneceſſary prolixity, becauſe every experiment-maker may obſerve it. And indeed I would trouble you with no more of this kind than is abſolutely neceſſary; but upon recollection that I have lately read a treatiſe upon attraction and repulſion by a member of the ſociety, before whom it is poſſible you may lay this letter, whoſe principal deſign is more particularly to explain the phænomena of magnetiſm, I muſt beg leave to obſerve upon what he has ſaid; becauſe it differs very much from what I think, and what I have now ſaid of it.

This learned gentleman ſays that magnetiſm is a continued ſtream of repellent fluid, circulating through the axis of the magnet, by entering at one end and going out at the other, and returning over the ſurface to enter again and continue the circulation. To prove this and make it evident to our ſenſe, he brings an experiment with the filings of ſteel ſprinkled on a ſheet of paper, and laid over the magnet to ſhew, by the figure in which they are ranged, the direction of the magnetic ſtream; and to make this more evident, he chuſes to make uſe of ſteel bars made magnetical, becauſe magnetiſm is found more regular in them than in the natural magnet. This experiment is very eaſily made by ſifting ſome ſteel filings, through a paper pricked with a pin, very thin upon a ſheet of paper laid over the magnetic bar, and putting the paper gently round the edges with the end of a quill, the filings are immediately ranged in order by the magnetic powers. But this order will never ſhew that they were thrown into it, by a ſtream of repellent matter, circulating through the axis of the magnet, from north to ſouth, and back again over the ſurface from ſouth to north; nor is there any thing in this order ſimilar to what this gentleman has deſcribed and delineated in his printed book. Therefore as the exiſtence of ſuch a ſtream is not proved by this experiment, I ſhall not meddle with this gentleman's abſtruſe reaſonings about it.

But as this experiment will help to explain what I have ſaid about the magnetic powers, I ſhall mention the order in which the filings are ranged by a ſingle magnet, and in ſome other complicated caſes when more magnets are uſed. And if I ſhould fail in deſcribing, the experiment will make it explicit. I have ſaid before that the magnetic powers are ſeparated in every bar rendered magnetical, and conſequently muſt be drawn toward the extremities of that bar; the point of greateſt power in either end of the bar is eaſily diſcoverable by ſuſpending a very fine needle, touched by that end of the bar, and bringing it over the axis of the bar, it will be drawn to that point; which is near a ſemidiameter of the bar, within the end of the bar; this power alſo extends, in the axis of the bar, toward the middle of the bar, leſſening by degrees. Now let us ſuppoſe this power in form ſomething like an egg, but longer and tapered toward the end next to the middle of the bar, with its rays thrown out every way perpendicular to its ſurface; this will nearly deſcribe the order in which the ſteel-filings are always ranged in this experiment; with this exception, that where the rays from the inward ends of the different powers ſhould croſs each other near the middle of the bar, they are there, by their mutual attraction, drawn into curved lines; which are more curved in proportion to their diſtance from the bar, as the angles in which they ſhould have croſſed, are leſs obtuſe. Theſe lines may make an haſty obſerver, think that there is a kind of circulation about the middle of the bar, and he may be more confirmed in that opinion by the ſuſpended needle, I juſt now mentioned, which will be thrown off from one ſide of the bar and drawn round to the other, in the direction of theſe lines; but let another needle be touched by a different end of the bar, and ſuſpended in like manner, it will be carried back over the ſame lines in a direction contrary to the firſt. Now if theſe needles are carried by the force of a circulating fluid, there muſt be two fluids circulating in direct oppoſition to each other; and it is very difficult to conceive how theſe needles can always keep the ſame different courſes, without ever being interrupted by the fluid moving in oppoſition thereto.

But lay two magnetic bars, parallel to each other, and about two inches aſunder, the north pole of one and the ſouth pole of the other, at each end, and ſift the filings on the paper over them, and you have a new ſet of circulations produced, if you ſuppoſe that the fluid circulates in the lines in which the filings are ranged; for here you have curved lines wherever the attracting rays ſhould have croſſed each other, both at the ends and about the middles, as in the former experiment, and from ſide to ſide reverſed to thoſe in the middle. But not to trouble you with a number of experiments which are eaſier made than deſcribed, I ſhall only ſay in general, that place the bars in what complicated ſhapes you pleaſe, the filings will always range themſelves according to the attraction or repulſion of the powers in the bars, and that in ſuch ſhapes, as no ways indicate a circulation of the magnetic power through the axis of the bar, and back again over the ſurface. Nor can I think there is any circulation between theſe different powers; for if there was, they muſt mix by their natural attraction, and then magnetiſm would ſeem to ceaſe, as it does when theſe powers are drawn together by rubbing them with other magnets in an order contrary to that which firſt ſeparated theſe powers in the bar. And to ſhew that theſe powers are really ſeparated in every magnetic bar, ſuſpend a touched needle by the eye and point, ſo that it may hang parallel to the bar lying on the table, carry this needle round the end and over the middle of the bar, you will find the north end of the needle attracted to the ſouth end of the bar quite round, and the ſouth end of the needle thrown off every way in the direction of the rays which I have deſcribed; then bring the ſouth end of the needle to the north end of the bar, and it ſhall be attracted quite round, and the north end of the needle thrown off in the direction of the rays. When the repelling ends of two magnetic bars are laid together, the needle will be carried round them and over the middle of the bars in the ſame order it is carried round one end of the bar. If you place two bars in a line with the north end of one to the ſouth end of the other, ſeparated to one third of the length of the bar (to which diſtance the powers ſeem to be ſeparated in moſt bars) and then ſift the filings over them, you will find them ranged between the bars, in the ſame manner they are ranged about the middle of each bar. Which ſhews that theſe powers, ſeparated to the ſame diſtance in the ends of two bars, act much in the ſame manner as when ſeparated in the ſame bar; except that they are weaker in their ends approaching the middle of the bar, as I have obſerved before.

I could peſter you with experiments with file duſt, ſuſpended needles, &c. but as magnetic bars are ſo common, I ſhall leave every gentleman to make experiments for himſelf, which will in the end (as far as I know) evince the truth of what I have ſaid; for I fear that I am already growing too prolix, which with many other faults, I muſt beg you will excuſe, as well as the want of proper deference and reſpect, due to you and the ſociety, in my manner of writing; for if I conceived I was in preſence of ſo learned a body of men, I ſhould be eaſily deterred from writing or ſpeaking; but the diſcoveries I am now laying before you are my own, and appear to me, by a thouſand experiments, to be founded in truth; and if no one elſe has hit upon the ſame things, theymay be worth the acceptance, as they tend to explain the phænomena of electricity and magnetiſm; and furniſh a new doctrine of both; which if you pleaſe may be called *Eeles*'s new doctrine of electricity and magnetiſm; not that I intend to include in this letter all that I know of theſe powers, for it may appear abſurd to ſpeak of their different actions according to their different circumſtances, or of their uſes in nature, before it is allowed that there are ſuch different powers.

I ſhall now mention my electrical experiment, and ſome few others, to explain what I have ſaid.

The experiment is this. Extend a ſilken ſtring horizontally, and from it ſuſpend two pieces of bog-down, by two ſilken ſtrings, of fifteen inches long, looped on the horizontal ſtring to ſlide backward or forward; then electrify the pieces of down, one with ſealing-wax and the other with glaſs; and when you bring them to approach within the ſphere of their activity, they will fly together, and upon contact all ſigns of electricity vaniſh; provided that the pieces of down were equally electrified, whether much or little; but if one was much more electrified than the other, then that which was leaſt electrified loſes all its electricity after contact, and the two remain electrified with the exceſs of electricity of that which was moſt electrified, which will appear from their being repelled by the wax or glaſs, which ever was uſed with the piece which was moſt electrified.

This experiment and many others led me to think that there muſt be two different powers in electricity, and that they muſt be differently excited by the wax and glaſs; accordingly I obſerved, that while a piece of down, ſuch as I uſed, was electrifying by wax or glaſs, that ſome of its fibres were violently extended from the electric, while others were as ſtrongly attracted to it; this made me think that the powers may be here divided; and that if I could draw off one power the down may remain electrified with the other; I then kept my finger in contact with the outward fibres, for a ſecond or two; and then plucked away glaſs and finger together, and had the pleaſure to find the down was electrified by the reſinous power, as appeared by its being attracted by the glaſs, and repelled by the wax. I then electrified the down in the ſame manner with the wax, and found that it remained electrified by the vitreous power, and was accordingly attracted by the wax, and repelled by the glaſs. This convinced me that my ſuppoſition was true. I then put the down toward the bottom of an excited wine glaſs, which was taper, ſo that the down was in contact with all ſides of the glaſs; and when it had remained there for a ſecond or two, I plucked the glaſs perpendicularly down from it, that it might not touch the mouth of the glaſs, and found that the down was electrified with the reſinous power. This I thought put the matter out of doubt. I then electrified the two pieces of down with the ſame glaſs; but one of them in the common way, with the vitreous power which acted out from the glaſs, and the other by the reſinous power which acted inward to the glaſs, by applying my finger as afore-mentioned, and found that they attracted each other in the ſame manner, which they did when electrified by the wax, and glaſs; and that after contact the electricity vaniſhed, provided they were equally electrified; but if one was more electrified than the other, then they both remained electrified with the exceſs of electricity in that which was moſt electrified; as may appear by their being both either attracted or repelled by the glaſs. And this I can always order as I pleaſe in the experiment. I made the ſame experiment with excited wax, and the effects were the ſame. I then ſuſpended a piece of down, from the ſilk ſtring, by a piece of linen thread about twelve inches long, and found that it could not be electrified with the vitreous power by the glaſs, nor with the reſinous power by the wax; the reaſon is becauſe there was more of that power, which acted inward to the electric, in the down, than there was of that power which acted outward in the thread. But reverſe this experiment by letting a piece of linen thread hang from one of the pieces of down which is ſuſpended by ſilk, and then touch the end of the thread with the glaſs or wax, and the down is immediately electrified; becauſe there is a great deal more of the power, which acts outward to the down and thread, than there is of the power, which acts inward, in that part of the thread which is in contact with or near the glaſs.

Theſe experiments and many others made me think it may be poſſible to make glaſs, by altering its ſurface, tranſpoſe theſe powers in the ſame manner that wax does. In order to which I took ſome coarſe emery, and obſcured one ſide of a pane ſaſh glaſs, which, when finiſhed, I excited by rubbing that ſide of the glaſs, and found the powers tranſpoſed at both ſides of the glaſs, in the ſame order as they are by excited wax; the reſinous power outward and the vitreous power inward. And they are again tranſpoſed into the different order at both ſides of the glaſs, when you rub the ſmooth ſide of the glaſs; and ſo, *vice verſa*, as often as you rub the different ſides of the glaſs. Which may be ſeen by rubbing the ſmooth ſide of the glaſs and electrifying a piece of down, which will be repelled by either ſide of the glaſs; then rub the obſcure ſide of the glaſs, and the down will be attracted by either ſide of the glaſs; rub the ſmooth ſide again, and the down will again be repelled by both ſides of the glaſs; and ſo on, attracted and repelled as often as you rub the different ſides of the glaſs, and three or four rubs at a ſide are ſufficient to tranſpoſe theſe powers. Or if you rub one half of the breadth of the glaſs at one ſide, and the other half at the other ſide, the piece of down ſhall be attracted at each ſide of one half of the glaſs, and repelled by each ſide of the other half of the glaſs. In theſe attractions and repulſions the down muſt not be ſuffered to touch the glaſs. But if it ſhould touch one half, and be electrified and repelled, then the other half will attract it.

When the obſcure ſide of the glaſs is rubbed, let two pieces of down hang near each other, and bring the glaſs under them, they will both be ſtrongly attracted to the glaſs, but ſome of their fibres extended from it; draw off one of them in ſuch manner that it may touch the upper fibres of the other in going off, and immediately after draw the glaſs from the other, and you will find theſe pieces of down electrified with the different powers; that which was firſt taken away by the power acting outward, and the other by the power acting inward. Note, in this experiment the down muſt not touch the glaſs, if it be within half an inch, it is ſufficient; and the ſame in other experiments where the outward power is to be drawn off by the finger or other non-electric. Sometimes when the down is very much attracted to and repelled from the glaſs, in its different fibres, if you withdraw the glaſs ſuddenly, there will not be the leaſt appearance of electricity in the down; becauſe the outward fibres were electrified by the power acting outward, and the inward fibres by the power acting inward, and theſe powers joining by their natural attraction after removing the glaſs, they are in their natural conjoined ſtate, in which they exert but little ſenſible action.

But if any one doubts whether theſe powers are really ſeparated in the down, while it ſtands thus attracted to, and repelled from the glaſs; let him bring a piece of excited wax to the repelled fibres, and they ſhall be attracted to it, and it will repel the fibres which are attracted to the glaſs; or let him bring another excited glaſs, and the upper or repelled fibres ſhall be repelled by it, and the lower fibres attracted to it.

The ſame effects will appear, in a contrary order, when the down ſtands ſo excited upon a piece of wax.

I may now appear tedious, but I have taken ſome pains to reduce my letter to theſe ſimple experiments to prevent objections to it; and I believe that I need not inform you that theſe diſcoveries were not made merely by the touch of a finger as I have deſcribed; for inventions coſt the inventors much more trouble, than can eaſily be believed by thoſe not uſed to invention, and who only ſee the thing when done.

I could now enter upon a number of complicated experiments, to ſhew how the natural quantity of electricity, in all bodies, muſt be acted upon to admit the adventitious, and various other things, relative to the complication of theſe powers. And alſo in magnetiſm, particularly to ſhew how the file-duſt may be diſpoſed in curve lines without any circulation between the magnetic powers; but I ſhall reſerve theſe matters for ſome future amuſement.

About two or three years ago I wrote ſomething to ſhew the cauſe of the refraction, &c. of light, according to my promiſe to the ſociety, to which I had added ſome anſwers to Sir *Iſaac Newton*'s queries; but upon meeting an advertiſement in the forty-ſeventh vol. of Philoſ. Tranſactions, that every man was left to his own credit, for the ſupport of his opinions publiſhed in the Philoſ. Tranſactions; and my opinion in that tract being contradictory to ſeveral great men, who wrote on that ſubject, I rather choſe to throw them by, than diſpute about them; not that I think them inſignificant, but I believe my credit very inſufficient to bear me through ſuch an oppoſition. I alſo wrote ſomething to explain the cauſe of the *Aurora Borealis*, and other things, which I may ſometime or other communicate for your amuſement. I am,

S I R,
Your moſt humble and
moſt obedient ſervant,

HEN. EELES.

Leſt I may be ſuſpected of boaſting of a greater knowledge in electricity than I really have, and that I would impoſe a new doctrine from a few ſimple experiments ; I ſhall ſet down a few of many hints, to which I am prepared to ſpeak, and alſo a few of many more complicated experiments which I have made.

Heat is as neceſſary as friction to excite the electrical powers to action. The electric powers muſt be ſeparated in the body to be electrified, as well as on the electric. This will explain the action and power of points in electrical experiments ; they do not act beyond the electric atmoſphere. The electric atmoſphere of the clouds is nothing ſimilar to what has been ſuppoſed. The electricity found in the air, only adheres to the vapours floating therein. Points of no great uſe in drawing off electricity from the clouds in time of thunder ; that being generally diſcharged by an exchange of powers from cloud to cloud, &c. ſometimes from the cloud to the earth, through the firſt conductor. The myſteries of Mr. *Muſchenbroeck*'s bottle eaſily explained by the ſeparation of the electric powers, and by their action when ſeparated. Theſe powers act freely through thin glaſs ; and where their paſſage through it is prevented, it is by one of the ſeparated powers, and not by the glaſs. All the complicated phænomena in different matters hanging on, or applied to the ſame *conductor*, may be explained by the action of the natural quantity of electricity in the conductor, according to the application of the adventitious. Mr. *Franklin's* ſuppoſition, or rather aſſertion, that the electricity which paſſes from the inſide of the electrified bottle to the outſide, during the exploſion, is lodged in the body of the glaſs, is quite wrong; for that electricity is not even in contact with the glaſs, while the bottle is charged; but on the contrary, a conſiderable part of the non-electric, which is in contact with the inſide of the glaſs, is electrified by the oppoſite power of electricity.

It may appear invidious in me to object to any opinion of this ingenious gentleman, becauſe we both happened to write hypotheſes to explain the cauſe of thunder; which opinions are now in the hands of the public, and muſt ſtand or fall by the judgment of the ingenious inquiſitive. I never heard of Mr. *Franklin's* name 'till ſome time after my laſt letter to you; I lately got and read his works, and if I wrote in a vindictive ſpirit, I could eaſily ſhew that his means of electrifying the clouds, his manner of bringing them into action to produce thunder, and his propoſed method of drawing the electricity from the clouds by pointed iron, are all wrong. But I think the world obliged to this gentleman, and many others who have taken ſo much pains to inveſtigate the nature of this ſubtile power. But as there is ſtill great room for enquiry, neither Mr. *Franklin*, or any ingenuous gentleman, ought to take it ill, that their errors are diſcovered ; becauſe authoriſed errors are the greateſt bar to knowledge ; witneſs the great deference paid to many of *Ariſtotle*'s abſurdities to this day ; nor ought any gentleman to think of impoſing on himſelf or others, for the vanity of ſupporting an opinion. And I ſolemnly declare, that if any of my opinions fall under a juſt cenſure, I ſhall be much obliged to him who ſets me right. I have ſaid ſo much, becauſe I think that what Mr. *Franklin* has produced in the 49th vol. of Philoſ. Tranſactions, page 300, as principles, will be found ſo far from being principles, that they are not ſtrictly true.

His firſt principle, that electric atmoſpheres repel each other, is ſo far true, that electric atmoſpheres of the ſame denomination repel each other ; but when they are of different denominations, they then attract each other.

His ſecond principle is in part true, and may be explained by the ſame means ; for if one body is more highly electrified than another body is by the ſame power, when the firſt approaches the ſecond, the exterior electricity of the firſt repels the exterior electricity of the ſecond, but, *per contra*, attracts the interior electricity of the ſecond ; which, if duly conſidered, will explain the effects of Mr. *Franklin*'s experiments.

As to his third principle, that bodies electrified negatively repel each other, it is erroneous ; for no body can be electrified negatively, as may appear from what I have ſaid in my letter, and ſhall ſay preſently; for the power greateſt in quantity always acts outward, and the body is electrified by that power.

Nobody will appear to be electrified while the electric powers are equal in or on that body; but if there be a greater quantity of one power than there is of the other power, then ſhall the greateſt quantity act outward from that body, and the body will be electrified with that power, and repel another body electrified in the ſame manner by the ſame power; but if the ſecond body be electrified by the other power, then ſhall they attract each other, and, after contact, all ſigns of electricity vaniſh, if they were equally electrified; but if not, both will remain electrified with the exceſs of electricity of that body, which was moſt electrified. All this appears in my firſt electrical experiment; but as the explanation of all electrical experiments depends upon rightly underſtanding this propoſition, that there are two different powers in electricity, and that they act in this manner, I would willingly be fully underſtood.

The ſame conductor may be firſt electrified by either of the electric powers, by an excited glaſs, and then by the other power, without altering any circumſtance in the conductor or glaſs, but merely by the manner of applying the glaſs to the conductor. The ſame piece of down may be electrified by an excited glaſs, ſo as to be attracted and repelled many times, as you think proper by the glaſs, without any freſh rubbing of the glaſs, or any other additional matter touching the down. The ſame conductor may be electrified with either or both the different powers in ſucceſſion by the excited glaſs, while in contact with the conductor, and all ſigns of electricity be withdrawn with the glaſs, without any other matter touching the glaſs or conductor. Two ſmall conductors, with a piece of down hanging on each conductor, may be ſo ſuſpended, that with one touch of an excited glaſs, one conductor ſhall be electrified with the vitreous power, and the other conductor with the reſinous power, and yet both pieces of down ſhall be electrified with one and the ſame power; and upon withdrawing the glaſs, all ſigns of electricity in the pieces of down, and in the conductors, ſhall diſappear; or by the manner of withdrawing the glaſs, you may leave both conductors and both pieces of down electrified with one and the ſame power.

A ſmall conductor, with a piece of down hanging from it by a linen thread, may be electrified through the down by one ſide of a pane of ſaſh glaſs, which is excited by rubbing the other ſide of the glaſs; and yet by continuing to rub the ſame ſide of the glaſs, all the electricity may be drawn back again through the glaſs from the conductor; and farther yet by continuing to rub the ſame ſide of the glaſs in the ſame manner, the down and conductor may be electrified with the power which is different from that which electrified them at firſt.

I could eaſily tire you with a recital of experiments to evince the truth of what I have ſaid; but much more ſo, were I to expatiate on the uſe of this combination of powers in the reflection and refraction of light, in the ſecondary attraction and repulſion in reſpiration and perſpiration, in evaporation and exhalation, in ſenſation, in vegetation, &c. but even the catalogue is too prolix to repeat, and lays open a train of enquiry too extenſive for my abilities, or inclinations to purſue; for to ſay truth, I have been more addicted to the purſuit of what the world calls pleaſures, than to philoſophical reſearches; however if what I write is any manner of entertainment to the Royal Society, or to you, I ſhall do myſelf the honour, at times, to continue a correſpondence.

To the Reverend Thomas
Birch, Secretary to the
Royal Society.

⁂ This letter was ſent, dated the 9th of *Auguſt*, 1757.

LETTER VI.

SIR, *Liſmore*, 14th *Jan.* 1758.

I WROTE to you on the 9th of *Auguſt* laſt, with a new doctrine of electricity and magnetiſm, but as I have not been favoured with an anſwer, I ſuppoſe that I have not made my doctrine intelligible; and indeed I am conſcious, from the ſcattered manner in which I have delivered my thoughts, and from not taking my experiments from the apparatus which is commonly uſed, that method, and a great deal more, is wanting to explain thoſe ſubjects fully. As to the want of method, I am only guilty of a general neglect; for I think that no one has yet attempted to give a theory from facts to explain the firſt action by which electricity is made ſenſible to us; I mean the attracting and then repelling any light matter, which is placed within the ſphere of activity, of the excited electric. If this method had been thought of, it would have led on to a more eaſy explanation of the effects of more complicated experiments. But I obſerve that a great deal of embarraſſment has ariſen from the adopting a few terms, which are unintelligible; ſuch as bodies being electrified negatively, and that glaſs throws out the electric fire, and that ſulphur and reſins drink it in; for how is it poſſible to conceive that two bodies ſhall repel each other the ſtronger, the more the repelling power is taken from them? or how can we ſuppoſe that glaſs, is always throwing out the electric power, and ſulphur always drinking it in, without one being ever full or the other empty? I think the adopting theſe terms has occaſioned the loſs of a great deal of refined reaſoning; but that I may not ſpeak too preſumptuouſly, I ſhall proceed to deliver, in part, what I have to ſay of electricity.

Electricity conſiſts of two different diſtinct powers, which always ſtrongly attract, and condenſe, each other, and therefore when they are in equilibrio, they exert but little ſenſible action; but when the equilibrium is deſtroyed by a diminution of one, and the encreaſe of the other, then the encreaſed power has a power of expanſion, in proportion to the ſubtraction of the other attracting power; by which means it forms itſelf into what we call an electric atmoſphere; which atmoſphere will repel any other atmoſphere raiſed from the ſame power, but attract any atmoſphere raiſed from the other power of electricity. Theſe atmoſpheres are always formed with an increment of rarity, as they recede from the leſſened attractive power, which leſſened power always acts inward to the body which is electrified. Theſe powers I have before called, only for diſtinction ſake, the vitreous and the reſinous powers. Theſe powers are ſtrongly attracted by all matter, and are always found adheſive to all matter,

ter, pretty near in proportion to the ſpecific gravity of the matter, as far as I have been able to judge. Nor is it eaſy to ſubtract any part of either of theſe powers from any part of matter, without adding an equal portion of the other power, which addition it will retain, until it is ſupplied with an equal quantity of the power firſt ſubtracted. So that when a ſpark is taken from the conductor, or any body electrified by excited glaſs, there is as much of the reſinous power paſſes from the finger into the conductor, as there is of the vitreous paſſing from the conductor into the finger; but of this in the experiments. I hinted in my letter that the electric powers muſt be ſeparated in the body to be electrified, as well as on the excited electric. That they are ſo, even before they make any exchange of power with the electrified body, will appear by bringing any body, without points and ſuſpended by electrics, into the atmoſphere of the conductor, but not ſo near as to ſnap, ſuppoſe within two or three inches, you will find the end next the conductor electrified with the reſinous power; and the end fartheſt from it electrified with the vitreous power; which may be tried by ſuſpending a piece of down by ſilk, and touching the down to one end, it will be electrified with the vitreous power, and will be attracted by the wax, and repelled by the glaſs; and upon touching it to the other end, it will be electrified by the reſinous power, and be repelled by the wax, and attracted by the glaſs. That this body is thus electrified by the electric atmoſphere of the conductor, by attracting one of the powers, naturally adhering to that body, and by repelling the other power, without making any exchange with the electricity of the conductor, may be thus proved. Bring the ſame body, diveſted of its electricity, (or, more properly ſpeaking, with its electric powers brought into an equilibrium ;) by touching it with your hand, or ſome non-electric, within the ſame diſtance of the conductor as before, and conſequently the different ends muſt be electrified with the different powers as at firſt, while it remains in that ſituation ; but remove it from the atmoſphere of the conductor, and bring the ſuſpended down to touch it ; you will find no ſigns of electricity in the body ; the powers then only acting upon each other are drawn into their natural condenſed ſtate all over the body, in which ſtate they exert but little ſenſible action.

This experiment may ſhew how the file duſt, ſprinkled on a paper over the magnet, comes to be ranged in curved lines, where the magnetic atmoſpheres attract each other into that form ; for every grain of duſt has its magnetic powers ſeparated by the magnetic atmoſphere, and their poles ranged in the ſame direction with the magnetic atmoſphere, conſequently they muſt attract each other, while they are magnetical, into the ſame direction with the magnetic atmoſphere ; and this may be, and is done without any mixture of the powers in magnetiſm, as it is in electricity; as I can prove by experiments. But to return and leave magnetiſm for ſome future amuſement.

I muſt here beg your patience while I add two or three more experiments, to ſhew that the natural quantity of the electric powers in all bodies is thus acted upon by an electric atmoſphere, while any body is immerged in it. For much depends on rightly underſtanding this matter.

Take a ſtrong wire ten inches long, turn the ends into ſmall loops, fix the middle to a glaſs handle with ſealing wax; from each loop ſuſpend a piece of down by a linen thread ſix or ſeven inches long; bring the handle perpendicular over the conductor, the wire at right angles with the conductor, and parallel to the horizon; as you lower your hand you will ſee the down ſtrongly attracted to the conductor, by the reſinous power being drawn into the down; and the vitreous power repelled into the wire; but when you approach the wire ſo near as to change powers with the conductor, the down flies off to each ſide. Diveſt the wire and down of the adventitious electricity, and bring it as before but the wire parallel to the conductor, when the down comes within two or three inches of the conductor, it will be ſtrongly attracted to the conductor, and by keeping your hand at the ſame diſtance and the wire parallel to the conductor, you may carry it quite round the conductor with the down attracted in all poſitions.

ons. But ſink your hand quite below the atmoſphere of the conductor, and then raiſe it with the wire parallel to the conductor, the wire will firſt approach the conductor, and therefore the reſinous power will be drawn into the wire, and the vitreous power repelled into the down, conſequently both pieces of down will be repelled by the conductor. Bring the wire endways toward the conductor, three or four inches above the horizontal level of the conductor, and you will ſee the piece of down next to the conductor, attracted to it, and the other repelled from it; the one being electrified with the vitreous power, and the other with the reſinous power; which may be proved by bringing a piece of excited ſealing wax, near either of them; but turn the handle that the wire may change end for end, and the powers will be immediately changed in the pieces of down; that next the conductor being always electrified by the reſinous power. But while the pieces of down remain one attracted and the other repelled, take a piece of wire in your other hand, and touch the wire ſupporting the down, you will ſee the fibres of the repelled down immediately drop and riſe again, the down will then be attracted to the conductor, and be found to be electrified with the reſinous power. The reaſon of this change is, that all the vitreous power in the wire and down is repelled into your arm by the atmoſphere of the conductor, and an equal quantity of the reſinous power drawn from you into the wire and down. Suſpend a ſmall cork ball with ſilk, that it may hang upon a level with the conductor, and within five or ſix inches of the conductor, it will remain quiet there; but if you touch it with the end of a wire held in your hand, the vitreous power of the ball is driven into you, and an equal quantity of the reſinous power carried to the ball, by which means the ball is ſtrongly electrified with the reſinous power, and is attracted with great rapidity to the conductor; and if your wire be any thing pointed and directed againſt the ball, the ſtream of reſinous power flowing from it, will keep the ball fixed to the conductor, by continually ſupplying it with the reſinous power; but when you remove the wire, the ball then parts with its reſinous power to the conductor, in exchange for the vitreous power, of which the conductor has the greateſt quantity, and the ball becomes electrified with the vitreous power, equally as the conductor is electrified, and is then conſequently repelled from the denſer part of the atmoſphere of the conductor to the rarer, the ball carrying its own vitreous atmoſphere with it. That all electric atmoſpheres, whether vitreous or reſinous, are rarer as they recede from the electrified body I ſhall prove preſently by experiment. Suſpend two cork balls, by linen thread, over the conductor, and reaching within two or three inches of the conductor; you will find them electrified by the reſinous power and repelling each other; but attracted to the conductor; for the vitreous power is driven from, and the reſinous power attracted into them, by the vitreous atmoſphere of the conductor while the wheel is turning; but ſtop the wheel and touch the conductor, and the balls fall together in their natural ſtate. Try the ſame experiment with ſmall balls ſuſpended by ſilk lines of ſix or eight inches long, the reſt of wire; you will find the balls electrified with the reſinous power, after ſome time turning the wheel; ſtop the wheel and touch the conductor, the balls will ſtill remain electrified; becauſe it required time and force to drive off the vitreous power through the ſilk, and to attract the reſinous power through the ſame; but when the atmoſphere of the conductor is removed, there is not any power to drive back the vitreous power through the ſilk to the balls. For here I muſt obſerve that ſilk, wax, glaſs and all electrics, have their powers ſeparated, and are electrified with the different powers, which are inherent in themſelves, at their different ends, while ſuſpended in an electric atmoſphere, in the ſame manner that non-electrics are; but not ſo ſuddenly; as their powers will not change place ſo quick; and in this ſlowneſs of changing conſiſts the difference of electrics and non-electrics. Suſpend a glaſs by clean ſilk over the conductor at three or four inches diſtance, and after ſome time turning the wheel, you will find the end next to the conductor electrified with the reſinous power, and the other end with the vitreous power; as will appear by touching each end with a piece of down, ſuſpended by clean ſilk. The ſame effects will appear in wax in the ſame circumſtances; but, more evidently if the conductor is electrified by a wax globe; but then the powers will change place, the vitreous power will appear next to the reſinous atmoſphere of the conductor, and ſo in all other experiments, when a wax globe is made uſe of. But I have all along ſuppoſed a glaſs globe to electrify the conductor. Glaſs and wax ſuſpended by ſilk may be electrified, as any non-electric is, by a wire communication from the conductor, which muſt remain for ſome time, and then be removed by a wax or glaſs handle.

From the different powers exiſting, and being capable of changing place in all electrics according to the power applied, you ſee the neceſſity of obſerving whether you may not be deceived by it, in your experiments. You alſo ſee the neceſſity of clean ſilk, wax and glaſs, otherwiſe a man may be deceived in many experiments; glaſs in particular, if it is ſo cold as to attract the damp of the air, is very apt to deceive. Let a piece of down hang by ſilk within four or five inches over the conductor, then with a wire, with a wax handle, electrify the down from the conductor, it will be repelled; then touch it with the end of a wire held in your hand, it will be attracted towards the conductor, as you purſue it to touch it, it will be highly electrified with the reſinous power, and ſtrongly attracted to the conductor, but repelled from your wire; the change of powers in the down muſt certainly be made through the wire, to and from your arm, the vitreous being driven into and the reſinous attracted from it, by the power of the vitreous atmoſphere in which your arm is immerged; and the down will retain its reſinous atmoſphere when carried quite away from the atmoſphere of the conductor. Tho' I fear I am growing tedious, yet I will add one experiment more, which I take to be an *Experimentum Crucis* in this caſe, which cannot be too much explained.

Take a cylinder of lead five, ſix or ſeven inches long, more than half an inch diameter, round off the ends ſmoothly, then ſaw it through the middle and bore a gimlet hole in each of the ſawed ends, fit a peg of dry wood tight into one hole and ſo that it may paſs eaſily into the hole of the other piece; ſuſpend each piece by a double piece of ſilk, the ends of which may be faſtened to the ends of two ſticks three or four inches long to prevent the cylinder from twiſting, &c. the ſilk ſhould be fourteen inches long or more; ſuſpend theſe two pieces, joined by the peg in the middle, as one cylinder, about two or three inches above the conductor, then remove them, ſtill joined together, quite out of the atmoſphere of the conductor, and bring a piece of down, ſuſpended by ſilk, to touch them, and if they ſhew no ſigns of being electrified, you have held them far enough above the

conductor;

conductor; then carry them back and hold them at the ſame diſtance from the conductor, and when they have remained there for a few ſeconds, lift up the upper piece and take it quite out of the atmoſpere of the conductor, and afterwards alſo the under piece without letting it come nearer the conductor in the removal, you will find the upper piece electrified with the vitreous power, and the under piece electrified with the reſinous power; for when a piece of down is touched to the one, it will be electrified and repelled by the wax, but when touched to the other it will be attracted by the wax; or the down will play between both 'till it reſtore the equilibrium, and no more ſigns of electricity appear; which proves that the pieces brought no electricity from the conductor, and that the powers naturally belonging to them were ſeparated by the atmoſphere of the conductor in the manner I have been all along deſcribing. Or let two cylinders touch each other; and all ſigns of electricity vaniſh upon reſtoring the equilibrium between the natural powers of the cylinders. If equal, but different, powers of electricity were only intimately mixed by their mutual attraction, all ſigns of electricity would vaniſh; but I ſhall ſhew that they are really condenſed thereby, and that they again expand themſelves in proportion to the attraction taken off by withdrawing any part of either of the powers.

From what I have been ſaying, I think it eaſy to explain how light bodies will be attracted, from the ſurface of a non-electric, to the conductor; for when they are brought within the atmoſphere of the conductor, their vitreous power and that of the body on which they reſt is repelled, and the reſinous power attracted, by which means the ſmall bodies become ſtrongly electrified with the reſinous power, and are therefore attracted to the conductor, 'till they exchange their reſinous power for the vitreous power of the conductor, and then they are repelled in the manner I have mentioned of the cork ball. But in other circumſtances where theſe ſmall bodies are placed upon a perfect electric (if ſuch a body were to be found, which I think is not to be found) or ſuppoſed to be free from all contact with any other body, with which they might exchange the electric powers to deſtroy the equilibrium, and conſequently where they muſt be attracted with their powers in equilibrio 'till they exchange powers with the conductor; the attraction is exceeding weak; and it will require a farther diſquiſition to explain the manner in which it is performed. From what I can gather from the actions of the electric powers it appears to me, that a vitreous atmoſphere repels another vitreous atmoſphere with a force exactly equal to that, with which it attracts a reſinous atmoſphere of equal denſity with the repelled vitreous atmoſphere. And that the attracting and repelling atmoſphere acts with a force proportional to, and encreaſing with, its own denſity. From hence it follows that a ſmall ſphere, whoſe electric powers are equal, ſuſpended by a perfect electric in the atmoſphere of the conductor, muſt have its powers equally attracted and repelled ; but as the reſinous power is attracted to, and the vitreous power is repelled from the conductor, the reſinous power comes into a denſer part of the atmoſphere of the conductor, and the vitreous power is driven into a rarer part of the ſame atmoſphere ; conſequently the reſinous power is more attracted by the denſer part of the atmoſphere, than the vitreous power is repelled by the rarer part of the atmoſphere ; and the differenee between this repulſion and attraction muſt be the force with which the ſphere is attracted to the conductor. It is eaſy to ſee that this attraction of a ſphere can be but very weak, and accordingly experiments evince the truth of this doctrine. Extend a wire horizontally acroſs your room at right angles with your conductor, but two feet above it ; and from wire hooks, to ſlide forward or backward on the horizontal wire, you may ſuſpend your balls. Suſpend a cork ſphere of half an inch diameter, with very clean ſilk, to hang upon a level with, and within three or four inches of the conductor ; turn the wheel and you ſhall ſcarcely perceive the cork to be atrracted ; but when the cork is placed near enough to the conductor to be attracted, the motion begins very gently, but the velocity encreaſes as it moves toward the conductor, notwithſtanding it is obſtructed by the gravity of the cork, which moves up in the arch of a circle to *which the* ſtring is radius ; but when the cork *parts with its* reſinous power to the conductor, it is repelled with great rapidity; and if it ſtrike againſt your hand or any non-electric in the atmoſphere of the conductor, which by that means has its reſinous power drawn towards the conductor, the cork becomes electrified with the reſinous power, and is attracted to the conductor, with a force exceedingly greater, than that with which it was attracted while the powers were equal in the cork. Suſpend another cork ball, exactly ſimilar to the firſt, by linen thread, hanging ſomething lower than the firſt, and placed twenty inches or two feet from the conductor, you will find it more attracted at that diſtance, than the firſt is at four inches diſtance; the reaſon is, becauſe the vitreous power is repelled from the ball along the thread, and the reſinous power attracted to the ball, by the vitreous atmoſphere of the conductor, by which means the ball becomes electrified by the reſinous power, and is more attracted by the conductor, tho' placed at ſuch a diſtance from it, and in a part of its atmoſphere ſo much rarer, than the ball ſuſpended by ſilk is; becauſe the powers remain in equilibrio in the latter, which therefore can only be attracted by the laws I have laſt mentioned. Place the ball, ſuſpended by ſilk, within four or five inches of the conductor, and that ſuſpended by thread within nine or ten inches of the conductor; then turn the wheel briſkly, the ball ſuſpended by thread will ruſh by that ſuſpended by ſilk, and remain fixed to the conductor as long as the conductor remains electrified with any force, and the ball on ſilk remains at, or very near its firſt diſtance. The ball on thread is fixed to the conductor by the continual ſtream of reſinous power flowing in from the thread, in the place of the vitreous power carried off by the thread. Place the balls at the ſame diſtance's again, but take off the cuſhion from the globe or cylinder, and with a rubber in your hand preſs the globe lightly to raiſe the atmoſphere gently, that the outer ball may be drawn towards the inner ball with a ſlow motion, and when it comes to touch it, or ſo near as to exchange powers with it, the ball on ſilk will be drawn into the conductor and fixed there by the reſinous ſtream flowing from the ball on thread; and if the atmoſphere of the conductor be neither increaſed or diminiſhed, the ball on thread will remain pretty near the place where it met with the ſilk ball; but if the atmoſphere of the conductor be much increaſed by rubbing the globe harder, then the ball on thread will be alſo attracted to the conductor, and both balls remain fixed there, provided that they lie in contact, that the ball on ſilk may be ſupplied with the reſinous power flowing in by the thread; but if they do not touch, then the ball on ſilk flies off, and the ball on thread remains fixed to the conductor. Cork balls, no bigger than peas, brought on a large flat glaſs which is quite dry and cold (for if a glaſs be warm, the powers are much ſooner ſeparated in it than when cold) and approached quick to the conductor, before the powers have time to change place in the glaſs, the balls will not be attracted to the conductor 'till almoſt in contact with it; but from a non-electric they will be attracted at a conſiderable diſtance. The reaſon why, I have given before. Take a cork ball of an inch diameter ſuſpended by ſilk; and a cork cylinder four inches long, and of the ſame weight with the ball, the ends rounded off, and ſuſpended by ſilk parallel to the horizon. Let them hang ſeparately within an inch and half or two inches of the conductor, that the end of the cylinder and ſide of the ball may be at equal diſtance from it; you will find the cylinder drawn into the conductor when the ball is ſcarcely moved; notwithſtanding that the center of gravity of the cylinder hangs an inch and half farther from the conductor than that of the ball. Which ſhews the electric powers being farther ſeparated in the cylinder than in the ball, that the difference of attraction and repulſion of thoſe powers by the vitreous atmoſphere of the conductor is greater in the cylinder than in the ball, becauſe they lie in greater degrees of the denſity and rarity of the atmoſphere of the conductor, and therefore the cylinder is more attracted than the ball. I could add more experiments to this purpoſe, but I believe theſe will be thought ſufficient; as I have other matters to touch on.

Though it may appear, from what I have been ſaying, that electric atmoſpheres have an increment of rarity, as they recede from the body electrified; yet, it may not be amiſs to bring an experiment or two, for farther proof. As to the ratio in which it increaſes I ſhall leave to others to amuſe themſelves with; but I think it will prove an endleſs ſearch; as it has done in magnetiſm, where there muſt be a different rule for every difference of ſtrength in the magnets which approach each other, or for every different degree to which the magnetic powers are ſeparated in the ſame magnet; the reaſon for which, I may, ſome time or other, trouble you with. Take a needle of four or five inches in length, ſupported on a pivott, like a compaſs needle, no matter whether it is magnetical or not; place it perpendicularly over the conductor, upon a non-electric point fixed to the conductor; this needle muſt be electrified with the ſame power with which the conductor is electrified, and, conſequently, their atmoſpheres muſt repel each other; and if the atmoſphere of the conductor was of equal denſity, there would not be any reaſon for this needle's reſting in one point more than another, after it had been whirled about; but you'll find, when it begins to ſtop, that it will traverſe backwards and forwards, until it fixes at right angles with the conductor; the ends thereof with their electric atmoſpheres, being ſtrongly repelled from the denſer part to the rarer part of the atmoſphere of the conductor. Arm the ends of this needle with two piſtol balls; the effects will be the ſame. Or, place a magnetic needle, in the ſame manner, upon a gun barrel, ſuſpended length-ways in the magnetic meridian; neither the directive power of the needle, or the attraction of its ends to the gun barrel, will prevent its being repelled from the denſer to the rarer part of the electric atmoſphere, and fixed at right angles with the barrel. Place a point upon a non-electric, that it may ſtand upon a level with the center of the conductor, and at ſuch a diſtance, that the balls on the ends of the needle, when placed upon the point, may come within a quarter or half quarter of an inch of the conductor. Here the needle is differently circumſtanced from what it was when placed on the conductor; for both ends are electrified with the reſinous power, and, conſequently, equally attracted to the conductor, while parallel to it; but when one ball begins to move, it is more attracted, by getting into a denſer part of the atmoſphere of the conductor, and by the other balls being leſs attracted in the rarer part of the atmoſphere; but moſt of all by the reſinous power being drawn into it through the point which ſupports it; and thus the ball gets into a conſiderable velocity by the time the needle comes to right angles with the conductor; and here a re-action would begin to obſtruct its circulation, if it were not prevented for ſome little time, while the ball exchanges powers with the conductor, and the conductor is again ſufficiently electrified to draw an equal quantity of the reſinous fire into the ball; but in this time the ball begins to recede from the conductor, with a velocity ſufficient to prevent the future re-action from ſtopping the ball, 'till the ball at the other end of the needle becomes neareſt to the conductor, and, conſequently, muſt be acted on as the firſt ball was. And thus, by an encreaſe of velocity in each turn, the needle ſoon comes to run an hundred miles in a minute. It is indifferent which way the needle begins to turn. But this needle will not circulate, placed upon a point in a dry glaſs ſtand; nor will it circulate at ſuch a diſtance as prevents its changing powers with the conductor.

Tie the two ends of a piece of ſilk near the two ends of a knitting-needle, ſo that, when you loop the middle of the ſilk, the two ſides and needle may make an equilateral triangle; ſtick a bottle cork on each end of the needle, and ſuſpend it by ſilk from the loop, that it may hang two inches above the conductor. When you electrify it with the vitreous power, of an excited glaſs, the corks are repelled from the denſer to the rarer parts of the vitreous atmoſphere of the conductor, and ſtand at right angles with the conductor; but then touch it with your finger, or wire, that it may be electrified with the reſinous power; or, for more ſatisfaction, electrify the needle with excited ſealing-wax, and the corks are immediately attracted from the rarer to the denſer part of the vitreous atmoſphere of the conductor, and the needle ſtands parallel to the conductor.

After all that has been ſaid of the wonderful power of points, I ſhall now ſhew that they do not act one inch beyond the electric atmoſphere; nor do they act upon that any farther than they are immerged in it; and indeed ſo far it acts upon them; by drawing the reſinous power in through them, and parting with ſo much of the vitreous power as comes in contact with them, but no more. Suſpend a piece of down by ſilk, ſo that it may hang againſt the ſide of the conductor; when you turn the wheel it will be electrified with the conductor, and fly off to the extreme part of the atmoſphere of the conductor: ſtop the wheel, and bring your point to the outſide of the down, inſtead of the down's being driven in toward the conductor, it will be attracted to the point, 'till it has exchanged powers with the point, and then it will be attracted to the conductor, and be electrified again, and repelled, and when it comes within a certain diſtance of the point, it will be attracted to it, as to any other non-electric; and ſo on, while the conductor remains electrified. When the down ſtands thus electrified on the atmoſphere of the conductor, immerge your point a little way into the atmoſphere, and you'll ſee the down approach the conductor, in proportion to the immerſion of the point, and ſo on, as often as you move the point forward to the conductor, but no farther, hold the point there as long as you pleaſe, which plainly ſhews that the point acts no farther than contact in drawing off the electricity. Indeed, while the wheel is turning, and the point immerged in the atmoſphere, there is a ſtrong ſtream of the reſinous power flowing in from the point to the conductor, and that in proportion to the vitreous power carried off by the point; (for the electric powers do change place more eaſily through points and ſlender bodies than when their ſurfaces are oppoſed to each other in any conſiderable quantity) and if this ſtream meets an electrified cork ball, or piece of down, it will change their powers, and electrify them with the reſinous power, by which means they are attracted to the conductor, and will be fixed there by the continual ſtream of reſinous power. But, draw back your hand into the rarer part of the atmoſphere, to leſſen the reſinous ſtream; and you'll ſee the down get off from the conductor by degrees, and hang between the two powers, without being forced into the conductor, or able to fly far from it; and upon this doctrine, of one power going out and another coming in, depends the ſuſpenſion of gold-leaf between an electrified plate, and a non-electric beneath. I dance half a dozen paper puppets, about two inches high, between my table and a large tin plate, wax-ed round the edges, and ſuſpended from my conductor; for, as the heads are electrified with one power, and the feet with the other, they are repelled at both ends, and, therefore, never come in contact, except the plate is raiſed high enough to let the lower part of one touch the higher part of another; and then they attract each other, and ſtick together. This dance has a droll appearance, and I have ſcarcely met with any one who could look on without laughing: but it requires ſome ſkill and practice to ſhape the puppets, for a very minute alteration will make them dance, or remain fixed to the top or bottom; for, the upper part muſt be ſo much larger than the lower part, as to contain a part of the power going in, as much greater than that going out, as will be equal to the gravity of the paper; but, from a little practice, I can make one of them dance for ſome minutes, without touching top or bottom.

But, to return to my points. It is evident, from all the experiments that can be made, that points have not any power to the electric atmoſphere at the diſtance of one inch; how is it then poſſible to ſuppoſe they can draw electricity from the clouds at a thouſand yards diſtance? for ſo far, and often much farther, is the ſeat of thunder generally placed from us. I know that very magnificent ideas of the extent of the electric atmoſphere of the clouds have been formed, and delivered to the public, as if they may extend to theſe points on or near the ſurface of the earth; but upon what foundation in reaſon theſe ſuppoſitions are ſupported, I muſt own I cannot ſee. For it is plain that the electric atmoſphere of a thunder cloud can only be a continuation of the electric atmoſpheres of all the drops; for we cannot ſuppoſe it one atmoſphere to one body 'till all the drops are joined in one body, and then indeed it may be very extenſive. For if we conſider how this electric atmoſphere is formed, the extent may in ſome meaſure be gueſſed at, at leaſt I think the method of computing will be reaſonable. If every particle of vapour carries up an electric atmoſphere proportioned to its diameter, after a coalition to form drops, the diameter of the electric atmoſphere of the drop muſt be in proportion to the diameter of the drop, as the diameter of the atmoſphere of the particle was to the diameter of the particle, which can never amount to this monſtrous extent. But I am running from my purpoſe, which was to explain the phænomena of electrical experiments, among which I think the charging and diſcharging Mr. *Muſchenbroeck*'s bottle has raiſed the attention of the inquiſitive; I ſhall therefore explain the myſtery of that phænomenon. In doing of which I think I ſhall ſhew that the electric powers, when in equilibrio, do really condenſe each other, and that the expanſion of one power always follows upon the withdrawing the attraction of the other power ; which expanſion is in proportion to the increaſe of one power and the diminution of the other power.

Place your bottle upon an electric ſtand with a communication from the conductor, then turn the wheel and you will find the bottle electrified on both ſides with the vitreous power ; which may be proved by touching them with the down ſuſpended by ſilk ; for when it is electrified by touching the outſide, it will alſo be repelled by the hook, or ball, which communicates with the inſide of the bottle ; when you apply a non-electric to the conductor, all ſigns of electricity are drawn from the conductor, but the bottle remains in ſome degree charged ; which degree will increaſe with, and be in proportion to the time of turning the wheel. Try this experiment with the communication to the coating of the bottle ; you will find the top of the bottle, and the outſide both electrified with the vitreous power, as before ; but when you diſcharge the conductor by touching it, the bottle is alſo entirely diſcharged. The reaſon of theſe different effects I ſhall explain preſently. The bottle I commonly uſe is a quart bottle filled with ſhot and water, with a wire thruſt through the cork and a piſtol ball at top ; the body is wrapped round with two or three turns of common ſheet lead ; for I obſerve it is neceſſary to have the non-electric on the outſide equal to that within ; and the more there is of both, the higher your bottle will charge. But the degree of charging depends upon the degree of ſeparation of the powers between the cuſhion and globe; and therefore much depends upon having a proper cuſhion. I have found a pig's bladder placed on linen or paper a very little damp, but not ſo much as to make the bladder moiſt, to anſwer well. But a moiſt or wet cuſhion will never produce a ſpark from the conductor; becauſe then the ſurface of the globe becomes non-electric by being wetted, in which caſe the powers cannot be ſeparated. So that I fear Mr. *Franklin*'s method of collecting electricity (which is the baſis of his famed hypotheſis) by rubbing the ſea-water againſt the ſalts which are diſſolved in it, will never do. I place my bottle on a round board, upon the necks of three clean bottles. From a bottle thus placed a wire communicates with the conductor, and another from the coating to the table; when the wheel turns you will not find any ſigns of electricity in the coating when you touch it with the bog-down; but the top is electrified with the vitreous power, and as often as you remove the wire communicating with the table, you will find the coating alſo electrified with the vitreous power, 'till the bottle is full charged. Then remove your wires from the conductor and table, and touch the coating with your finger; the down will hang ſuſpended by it without any appearance of being electrified; then touch the bullet with your finger, and the down will be ſtrongly repelled from the coating, and be electrified with the reſinous power. Then touch the bullet with another piece of ſuſpended down, and the down will be electrified with the vitreous power and be repelled; then touch the coating with your finger, and the down is repelled much farther by the bullet; but the down which was repelled from the coating is now attracted to it, and remains quiet there, till you touch the bullet with your finger, and then it is electrified as at firſt and violently repelled, and the down which was electrified by the bullet is now attracted towards it. And this change in the extent of the atmoſphere of the different powers is almoſt inſtantaneouſly made, as often as you touch the bullet or coating. Or you may place your wire from the bullet to the conductor, and ſuſpend a piece of down to touch the conductor; when you touch the coating the down is repelled from the conductor, and that next to the coating is attracted to it; then touch the bullet, and the down is repelled from the coating and attracted to the conductor; and ſo on, as often as you touch the bullet and coating. From hence, I think that two things evidently appear; firſt, that the bottle is electrified with the vitreous power on the inſide, and with the reſinous power on the outſide. Secondly, that when the balance of theſe powers is deſtroyed by leſſening the quantity of one, the extreme part of the other expands itſelf into an extenſive atmoſphere, and that the atmoſphere of the leſſened power is condenſed, as appears by the down's falling cloſe into the conductor or coating. But here a more myſterious phænemenon appears, which has not been taken notice of, (nor indeed was it poſſible to take notice of it, until it was underſtood that there are two powers in electricity) that the extreme parts of the vitreous and reſinous atmoſpheres meet on the outſide of the bottle without attracting each other ſo as to mix. Which I think may be thus accounted for. The electric powers are attracted to matter (the bottle) according to the quantity of matter, therefore the moſt condenſed and attractive parts of theſe powers muſt meet at the glaſs; and the attraction of each proceeds through the ſeries of particles of the other power to the extremes, conſequently the extremes cannot attract each other with a greater power than they are attracted themſelves by the denſeſt and moſt attractive part of the oppoſite and attracting power at the glaſs. It now remains to ſhew how theſe powers came to be ſituated on the inſide and outſide of the bottle; or why they do not mix through the glaſs where their ſtrongeſt attraction is. We muſt firſt conſider the ſeparation of theſe powers between the globe and cuſhion, for, according to the degree of ſeparation there, all the reſt muſt follow. The globe parts with its reſinous power to the cuſhion in exchange for the vitreous, the conductor in like manner to the globe, and the inſide of the bottle in like manner to the conductor; *and ſo* the exchange would go on with the next non-electric, but that the bottle gives ſome obſtruction to the paſſage of the electric powers; by which means the vitreous power which paſſes through the glaſs, or round by the outſide of the bottle, to the non-electric upon the outſide of the bottle, is carried off, together with the vitreous power of the coating, along the wire which communicates with the table, in exchange for an equal quantity of reſinous power brought back by the wire to the coating of the bottle; 'till at length the reſinous power on the outſide becomes a balance in attraction to the vitreous power on the inſide; by which means the reſinous power is drawn from the inſide of the bottle to the conductor; ſo that the bottle remains a partition between theſe powers; and they cannot change place through the fine pores of the bottle, while their ſurfaces are oppoſed to each other in ſuch quantity. For you ſee when the juncture is made in open air, where their ſurfaces are oppoſed in any quantity, it is not done without violence; for when you compleat the circle with flat ſurfaces a little convex in the middle, the conſequence is a loud crack and flaſh of fire, by burſting through each other. For where the different powers attract each other in any quantity, they are much condenſed, which I could prove by experiment. This would alſo prove other matters which I would willingly ſpeak to, but I have not room for every thing. The violent convulſion felt through the body, by com*pleating* the circle with the hands, is only *occaſioned* by the different powers paſſing in oppoſition through the ſame nerves. For let one man touch the coating, and another touch the top of the bottle, the bottle will be diſcharged without ſhocking either of them; tho' it is evident that as much fire paſſed through each, as if each had ſingly diſcharged the bottle. But in this caſe the fire is diffuſed through all parts of the body, and the fire brought in is drawn from all parts of the body, therefore the nerves cannot be ſo much ſhocked as in the firſt caſe, where all the fire paſſes, in oppoſition, through the ſame nerves. The reaſon why the bottle ſet on a glaſs ſtand, in the firſt mentioned experiment, without any communication with the table, remains charged after turning the wheel for ſome time, and then diſcharging the conductor; is becauſe the vitreous power at the outſide drew up ſome of the reſinous power from the table through the ſides of the glaſs ſtand (for I ſhewed before that there are not any bodies perfectly electric, and that the powers may be changed in glaſs) or perhaps from the damp of the air, &c. but this adventitious reſinous power has no way of retreating upon diſcharging the conductor of its electricity; and therefore by its attraction of the vitreous power in the bottle retains an equal quantity of it there, by which means the bottle is ſo far charged, and remains ſo 'till diſcharged in the common way. But in the ſecond experiment where the wire from the conductor is fixed to the coating, whatever reſinous power is drawn from the table to the coating paſſes on in exchange to the conductor, globe and cuſhion, and there being no way for the reſinous power to come at the inſide of the bottle; upon diſcharging the conductor, the bottle is found entirely diſcharged, with its powers in equilibrio in their natural ſtate. But the moſt myſterious phænomenon of this bottle ſtill remains, without any notice being ever taken of it, that I can find; which is, that after the bottle has been highly charged, and then all communication removed, and the conductor diſcharged that no electric atmoſphere may be near the bottle; after you diſcharge the bottle ſo far that no ſigns of electricity appear upon touching the coating with the finger of one hand, and the bullet with the finger of the other hand; yet upon letting the bottle remain two or three minutes upon the ſtand, you will find it in ſome meaſure charged again, ſo as to yield a ſpark and give a ſhock to be felt in the wriſt; and after that diſcharge let it reſt three or four minutes more, you will find another charge that may be felt as far as the knuckle or back of the hand, upon diſcharging the bottle with a finger of each hand. And thus by letting it reſt for longer intervals you will find it revive for five or ſix times, the ſparks growing ſmaller in each ſucceſſion. If the bottle be diſcharged with a wire fixed to a glaſs or wax handle, the revived charge (as I may call it) will be the ſtronger. This effect

I muſt own at firſt ſurprized me, and ſeemed very difficult to explain; but I now think it may be explained by the laws which I have given, and accounted for in this manner. I think that it may be eaſily conceived from the manner in which a bottle is charged, that there is a greater quantity of the electric powers affixed to a bottle highly charged, than would be affixed to that bottle by its mere attraction of thoſe powers by its quantity of matter. When this bottle is diſcharged by a free communication from the inſide to the outſide, theſe powers are equally mixed among each other, that is equal quantities of reſinous and vitreous withinſide; and equal quantities of reſinous and vitreous withoutſide; and the quantities of both equal withinſide, and withoutſide. They then attract each other equally, and therefore a communication from one ſide to the other, produces no change in theſe powers, and therefore no ſenſible action. But theſe powers being more than the bottle can hold by attraction, one of them begins to move off from the outſide to the adjacent matter, and upon that ſubtraction, a different power alſo gets off from the inſide, in proportion to that which has quitted the outſide; now tho' the quantities within and without are equal, yet they are not equal as to themſelves, that is, there is more of the vitreous than of the reſinous within, and more of the reſinous than of the vitreous without; ſo that when a communication is formed, the bottle appears to be charged according to the diſparity of the powers within and without. Thus this exchange goes on to the fifth or ſixth ſucceſſion, or farther according to the intervals of each trial; but if time ſufficient be allowed at firſt, the equilibrium will be reſtored in one diſcharge.

There are many complicated caſes of this bottle which I think have not been taken notice of, two or three of which I will mention. Firſt, when the bottle is fully charged by the wire, and the wire removed from the table; you may ſhift the communication with the conductor, from the top of the bottle to the coating, and by continuing to turn the wheel, the reſinous power, with which the coating is electrified, becomes covered with the vitreous power, and you may take as many ſnaps from it as you pleaſe; or electrify as many bodies by it with the vitreous power as you think proper, yet it makes no alteration in the charge in the bottle: for when you ſtop the wheel and remove the communication with the conductor, and touch a finger to the outſide of the bottle, all ſigns of the vitreous power diſappear; and the reſinous power appears upon the coating, when you touch the bullet with a finger; and when the circle is compleated, the bottle is diſcharged with as loud a report as it would have done before you applied the communicating wire to the coating, for the vitreous power within the bottle being undiſturbed, kept an equal quantity of the reſinous power firmly fixed to the outſide of the bottle. But when the bottle is fully charged, and you ſhift the wire from the conductor, to the coating, from the top of the bottle, after the wheel has turned ſome little time; if you attempt to take a ſpark from the bullet, you will receive it with violence at double the diſtance you can draw a ſpark from the conductor; the reaſon is, that the vitreous power, covering the reſinous power on the outſide of the bottle, takes off the attraction of the reſinous power from the vitreous power at the inſide, and therefore leaves that power with greater freedom to fly off; but as you take the ſnaps the ſparks leſſen at every ſnap; becauſe the vitreous power in the bottle is leſſened, and the reſinous power, within, encreaſed by the quantity taken in, in the exchange on every ſnap. And thus in a few ſnaps the bottle is diſcharged; but if you go on to take more ſnaps, the bottle is recharged with the reſinous power in the inſide, inſtead of the vitreous with which it was charged before. Here is another caſe. Suppoſe an hundred turns of the wheel will charge your bottle; turn fifty of them, and then remove the communication between the coating and table; as you turn on, (whether you continue the communication from the conductor to the top of the bottle, or ſhift it to the coating) you will find the bottle electrified with the vitreous power within and without; then with wax or glaſs, remove the wire which communicates with the conductor; and with a wire, fixed to a wax or glaſs handle, diſcharge your bottle; there will be as loud a report as you were to expect from fifty turns; but the bottle will remain electrified with the vitreous power, both within and without; and this electricity will be diſcharged both within and without, by touching either the bullet or coating with your finger. Here I could add much in regard to the paſſage of the electric powers through glaſs; and various other matters. But if I were to explain all that occurs I muſt write a long letter indeed. But as it is reaſonable to ſuppoſe that you will now be pleaſed to come to the end of this, I will conclude with requeſting that any of the ingenious gentlemen of the ſociety, who have been converſant with electricity or magnetiſm, will tell me honeſtly where they think I am wrong, or where I have been deficient in explaining my meaning, or to communicate ſuch effects as they think cannot be deduced from the laws of electricity which I have laid down; or that they would be ſo good as to communicate any thoughts of their own towards eſtabliſhing ſome fixed doctrine of electricity, that we may not be led any longer by vague conjectures, but be enabled from ſome fixed laws to deduce the uſe of electricity in the operations of nature. I have obſerved Mr. *Watſon*'s and Mr. *Canton*'s names in the Philoſ. Tranſactions, I ſhould be obliged to theſe gentlemen in particular; for I live in a neighbourhood where there is not a ſingle man, who has any taſte for reſearches of this kind; and as cuſtom ed, as we call it; and therefore the particles detached from them may bring off the electric powers affixed to each particle in very different quantities. And thus, my ſyſtem of the aſcent of vapour and exhalation, thunder, rain, &c. may be better explained than before. But this would require a paper by itſelf; therefore, for the preſent, I acquieſce.

I believe, I ſhould have mentioned the cylinder I uſe, becauſe it is but ſmall in proportion to many others, that the experiments may be better underſtood; and, alſo, to ſhew another circumſtance in the eaſy fitting up of globes, or cylinders. Mine is a clear glaſs bottle, four inches and an half diameter, and ſeven inches long in the ſide, with an iron axis paſſed through the middle, which does not prevent the working of the bottle; for the ends of the bottle, the iron axis, and the pikes of the lath, upon which it turns, are all electrified with the reſinous power, while the wheel is turning; which ſhews that the vitreous power does not fly off that way: and a communication from the pike to the coating of the bottle, will make the bottle charge ſooner and higher, than a communication with any other non-electric; becauſe the reſinous power is there prepared to fly off to the bottle.

Here, I could tell how heat excites, and friction ſeparates the electric powers; but this would require a recital of experiments too prolix to inſert.

I muſt beg the favour that you will let me know, if this letter gets to your hands, or whether you received mine of the 9th of *Auguſt* laſt.—Wherever I mention down, I mean bog down, and not the down of feathers, which will not anſwer in theſe experiments, being itſelf an electric *per ſe*.

⁂ Sent this letter, dated 14th *February*, 1758, to the Reverend *Thomas Birch*, ſecretary to the Royal Society, in Crane-court, Fleet-ſtreet, London.

LETTER VII.

SIR, *Liſmore*, 3d *January*, 1760.

I SEND you by this poſt a letter for the royal ſociety in two pacquets, and the incloſed. My deſign in this letter is to give a farther explanation of the cauſe of thunder, and of the deſcent of vapour and exhalation, &c. But as my reaſoning in this letter is founded on a doctrine of electricity, which I communicated to you in a letter of the 14th of *February*, 1758, and that I find that that letter is not inſerted in the Philoſo. Tranſactions, I am doubtful that this letter may be thrown by with the ſame inattention. All I can ſay is that I am fully aſcertained, by numberleſs experiments, of the truth of that doctrine; and that I am perſuaded, that, if any of the gentlemen of the Royal Society had examined it carefully, they alſo would have found it to be true. I may add farther, tho' it may appear preſumptuous in me, that it is the only doctrine of electricity extant, (which I know of) which can fully explain the electrical phænomena. For what Mr. *Franklin* has ſaid, (tho' an ingenious, and I believe an ingenuous gentleman) that electricity reſides in glaſs in ſuch a manner that when one ſide of the glaſs receives an additional quantity of electricity, (which he always ſuppoſes that it does, when it is electrified; tho' this is alſo a miſtake) the other ſide always parts with an equal quantity of its natural ſhare of electricity; ſo that, according to his explanation of charging the *Leyden* bottle, one ſide muſt be electrified negatively, or minus; that is, want as much of its natural ſhare of electricity, as the other has over and above its natural ſhare. I ſay, that this is all a miſtake; for glaſs may be electrified at both ſides, at the ſame time, either by the vitreous or reſinous power: and, therefore, in the firſt caſe, both ſides muſt abound with electricity, and, in the next, both ſides muſt want of their natural ſhare. Which quite overſets his doctrine of electricity in glaſs. A thin plate of wax may alſo be electrified at both ſides at the ſame time, by either of the powers. And, what is more, I can electrify the glaſs in either manner, by electrified wax; or, I can electrify the wax in either manner, by electrified glaſs; or, I can leave the different ſides of the wax, or glaſs, electrified by the different powers of electricity at the ſame time. Which matters can never be explained by poſitive and negative electricity, or a plus and minus of the ſame power. Nor can any of the phænomena of electricity be explained by the action of one ſingle power; ſince it is evident that they depend upon the actions of two different powers, and their manner of acting on each other.

Mr. *William Watſon* in his experiments in *vacuo*, in the 47th vol. of Philoſ. Tranſ. thinks that he ſees the electricity paſſing in through his *vacuum* to the machine; but in this he ſees but in part; for there is as much of one power going out as there is of the other coming in; for theſe powers are never rendered viſible but in paſſing from one body to another in oppoſition to each other. And this he may try by placing his *vacuum* upon wax, and applying the wire of a *Leyden* phial, while the coating is held in his hand, to the farther end of his *vacuum*, and he ſhall find the phial charged with the power which comes out from the machine, which will be contrary to that which goes in. And I am confident that if I were with the gentlemen of the Society, I could eaſily convince them of the truth of my doctrine. All I require is, a careful examination of it; and that, if in any part of it I appear unintelligible, I may have an opportunity of explaining the matter more fully.

As the electric powers are a great principle of motion in the world, it is certainly worth the while to enquire into the truth of a doctrine which ſo eaſily explains all the known phænomena of electricity in its natural and artificial operations; which, without it, are inexplicable.

As I do not know that any writer upon electricity has conſidered it as conſiſting of two different diſtinct elaſtic powers, I am the leſs ſurpriſed that my doctrine is not readily accepted; but ſurely, if it be true, it is not leſs a truth for my having ſingly hit upon it. A truth which will greatly conduce to ſpeculative knowledge, and that it may be applicable to practical knowledge, I have often experienced, particularly in the curing of the human body.

I ſhall only trouble you with one experiment more, which, if any of the writers on electricity can explain by any thing which I have ſeen publiſhed about poſitive and negative electricity, or a plus and minus of the ſame power, or by glaſs throwing out and wax drinking in the electric power, or by any ſuppoſed action of one ſingle power, I ſhall readily grant that I have troubled the Royal Society, with my doctrine of two different diſtinct elaſtic powers, to very little purpoſe.

I have two cylinders, one of glaſs and one of ſealing-wax; each about five inches diameter. Which cylinders I can make exchange officers as often as I pleaſe; that is, I can make the glaſs throw out the reſinous power, and electrify the conductor and all other matters communicating with it in the ſame manner that wax generally does; and I can make the cylinder of ſealing-wax, throw out the vitreous power, and electrify the conductor and all other matters communicating with it, in the ſame manner that glaſs generally does. Which ſhews, that both the different powers exiſt together both in the wax and in the glaſs, ſince I can alternately throw out which power I pleaſe, either from the wax or from the glaſs.

I can, with a rubbed piece of ſealing-wax, electrify a piece of bog down ſuſpended in ſilk with the vitreous power, and then retract that and electrify it with the reſinous power, and thus change the powers in the down ſeveral times without any freſh rubbing of the wax, or touching it to any other thing, which demonſtrates that both the powers exiſt together withoutſide the ſurface of the wax. Nor ſhall a ſpectator perceive any difference in my manner of touching the down with the wax. Theſe facts I have ſhewn to hundreds of people; who can atteſt the truth of what I write; and many more experiments, which demonſtrate that two different diſtinct elaſtic powers of electricity do exiſt together in all bodies.

And upon theſe powers being equally attracted by all matter, and their equal and ſtrong attraction and condenſation of each other, and their great elaſtic power, depend all the phænomena of electricity; and this will be found true while things ſubſiſt in their preſent order. For the Creator eſtabliſhed the principles of motion and reſt invariable; for ſhould they alter or ceaſe, all muſt fall into wild diſorder and confuſion.

And now, ſir, I muſt beg leave to addreſs you in your private character, and to requeſt a favour which I hope you will not refuſe, which is only to let me know whether my letters of the 9th of *Auguſt*, 1757, 14th of *February*, 1758, and 26th of *February*, 1760, got to your hands. For if you received them, and they were not thought worth the attention of the Royal Society, it may be impertinent to trouble them, you, or myſelf with any more eſſays on this ſubject, tho' it might well employ many hands for much more time to come.

I have been applied to by ſome friends to write ſomething ſyſtematical on electricity, and particularly by ſome phyſicians, to explain my method of applying the electrical powers to medicinal uſe; but if my papers are accepted by the ſociety, I ſhould rather chuſe to write eſſays than ſyſtems; but if not, I ſhall endeavour to leave my diſcoveries and opinions of theſe matters to the world in another courſe.

I am, SIR,
your moſt humble and
moſt obedient ſervant,
HEN. EELES.

⁎⁎⁎ Sent the foregoing letter, dated 20th of *March*, 1761, to the Reverend *Thomas Birch*, Secretary to the Royal Society.

LETTER VIII.

Liſmore, 14th *October*, 1760.

GENTLEMEN,

HAVING lately received the volume of Philoſ. Tranſactions for the year 1757, I was ſurpriſed at meeting with two letters ſigned *Eraſmus Darwin*, Phyſician at *Litchfield*, *Staffordſhire*, beginning at page 240, which that gentleman is placed to call a confutation of what I wrote to you on the cauſe of the aſcent of vapour and exhalation. Indeed with the liberties which he has taken, he may confute or rather confound any author that ever wrote. But I believe this gentleman is but a young philoſopher, for he appears more ſtudious to make a rhetorical flouriſh, than to explain the matter which he writes about. I ſhall beg leave to tranſcribe his firſt paragraph, becauſe I think it ſounds well, tho' not much to the purpoſe.

‘ There is ever ſuch a charm attendant up-
‘ on novelty, that be it in philoſophy, medi-
‘ cine, or religion, the gazing world are too
‘ often led to adore, what they ought only
‘ to admire: whilſt this vehemence of enthu-
‘ ſiaſm has generally ſoon rendered that object
‘ contemptible, that would otherwiſe have

‘ long

‘ long laid claim to a more ſober eſteem. This
‘ was once the fate of chymiſtry; the vain
‘ and pompous boaſts of her adepts brought
‘ the whole art into diſreſpect; and I ſhould
‘ be ſorry, if her ſiſter electricity ſhould ſhare
‘ the ſame misfortune. It is hence the ingenious
‘ Mr. *Eeles* will excuſe me, for endeavouring
‘ to lay before you my opinion on the
‘ aſcent of vapours, tho’ it by no means coincides
‘ with that he is ſo ſtrenuous to eſtabliſh,
‘ and plucks a plume from his idol goddeſs,
‘ electricity.’

Here I think this gentleman might have diſtinguiſhed better between art and nature; it is true that an art may ſuffer by the imprudence of its profeſſors; but dame nature will go on with her work, whether we talk wiſely or otherwiſe about her; and therefore he may lay aſide his concern. He has indeed told me two things which I did not know before, that electricity was a feathered goddeſs and an idol of mine; ſince he will have it ſo, he may have a plume for his pains when he can pluck it.

He begins by telling you that I have ſaid, that every particle of vapour is endued with a portion of electric fire; and that there is no other ſufficient cauſe aſſigned for their aſcending. It is true that I then thought ſo, and ſtill do think ſo, and had I known of any ſufficient cauſe aſſigned, I muſt have been a moſt ignorant, impudent blockhead, to think of palming any other on the Royal Society. He then goes on thus.

‘ My deſign is therefore firſt to attempt to ‘ ſhew, that another theory, founded on prin‘ ciples better known, will ſufficiently explain ‘ the aſcent of vapours: and then, that ſome ‘ kinds of vapours are not endued with a more ‘ or leſs than their natural ſhare of electric ‘ æther.’

The latter part of this deſign I ſhall readily grant him is true, but for ſome reaſons which this gentleman ſeems a ſtranger to; that is, that all bodies have a quantity of electricity nearly in proportion to their ſpecific gravities; and that bodies do not become electrified by having a greater or leſs portion of electricity in general; but by an encreaſe of the one, and a proportional leſſening of the other power of electricity; for when the attraction of the leſſened power is diminiſhed in proportion to the quantity of the power; the encreaſed power expands itſelf into an extenſive atmoſphere; which is generally called electricity. So that thoſe who talk of an accumulation of electricity, do not underſtand the action of theſe powers. This matter I have explained in a letter to Doctor *Birch*, of the 14th of *February*, 1758, and there given the general laws by which theſe powers act on each other, and on the other parts of matter.

As to this gentleman's new theory founded on well known principles; I do think that it was well known that fire made the pot boil, and that the ſteams aroſe; but when he comes

to

to ſhew us how, it is by the immeaſurable expanſion of water in ſteam which renders it ſpecifically lighter than air; but I believe that he is the firſt philoſopher who ever did, and perhaps the laſt that ever will take ſteam, vapour, or cloud to be one continued expanded maſs of water; for he himſelf in the next page ſpeaks of the ſpherules and globules of vapour. In my letter concerning the cauſe of the aſcent of vapour, &c. I have demonſtrated that the ſpecific gravity or expanſion of a body is not altered by dividing that body into minute parts; therefore, until he has ſhewn that every particle of vapour is a thouſand times more expanded than it was while a conſtituent part of the watry maſs; it is impoſſible for him to ſhew that it is ſufficiently ſpecifically lighter than air, ſo as to aſcend in it by the laws of ſpecific gravity. And here I may reſt this matter; but as there is ſomething extraordinary in this gentleman's manner of reaſoning, I beg leave to make a few obſervations.

Firſt, he does not make any diſtinction between the expanſion of the parts of bodies, or their being ſet at a diſtance from each other by ſome repulſive power; ſo that the immeaſurable expanſion and condenſation of vapour, which has puzzled him, may not be in the watery particles, but in that ſubtile power which keeps them aſunder. And that it is ſo, I think I can eaſily prove from the nature and actions of the electric powers; which powers will fully anſwer almoſt all the purpoſes (except that of gravity) for which the great Sir *Iſaac Newton* ſought for ſuch ſubtile powers of attraction and repulſion. And here I muſt beg leave to tranſcribe a few lines from that great author, where he ſuppoſes the neceſſary exiſtence of ſuch powers; eſpecially of repulſion.

"And that there is ſuch a virtue, ſeems to "follow from the reflections and inflections of "the rays of light. For the rays are repelled "by bodies in both thoſe caſes, without the "immediate contact of the reflecting or in-"flecting body. It ſeems alſo to follow from "the emiſſion of light; the ray, ſo ſoon as it "is ſhaken off from a ſhining body by the vi-"brating motion of the parts of the body, and "gets beyond the reach of attraction, being "driven away with exceeding great velocity. "For that force which is ſufficient to turn "it back in reflection, may be ſufficient to "emit it. It ſeems alſo to follow from the "production of air and vapour. The parti-"cles when they are ſhaken off from bodies "by heat or fermentation, ſo ſoon as they "are beyond the reach of the attraction of "the body, receding from it, and alſo from "one another with great ſtrength, and keep-"ing at a diſtance, ſo as ſometimes to take "up above a million of times more ſpace than "they did before in the form of a denſe body; "which vaſt contraction and expanſion ſeems "unintelligible, by feigning the particles of

" air to be ſpringy and ramous, or rolled up " like hoops, or by any other means than a " repulſive power. The particles of fluids " which do not cohere too ſtrongly, and are " of ſuch a ſmallneſs as renders them moſt " ſuſceptible of thoſe agitations which keep " liquors in a fluor, are moſt eaſily ſeparated " and rarified into vapour, and in the lan- " guage of the chymiſts, they are volatile, ra- " rifying with an eaſy heat, and condenſing " with cold." *Newton*'s 31ſt Query.

Now it is evident that this great man never imagined that this vaſt expanſion and contraction of vapour was in the particles of vapour, but in that power which ſet them at a diſtance from each other. If the particles are not expanded, how do they become ſpecifically lighter than air? If Mr. *Darwin* takes in the repulſive power to make out the expanſion, he then proves my ſyſtem; if not, he has proved nothing toward the cauſe of the aſcent of vapour.

And here, gentlemen, be pleaſed to obſerve the great conformity that there is between what this great man has ſaid about the production of vapour, and what I have ſaid on the ſame ſubject, in my letter concerning the aſcent of vapour; tho' he only reaſoned from the effects to a ſuppoſed cauſe, and I reaſoned from a known cauſe to the effects, without having his doctrine in view; nor do I recollect that I ever read his queries till long after my writing that letter; when they were occaſionally put into my hands by an ingenious gentleman, who had read Sir *Iſaac*'s works many years later than I had done. As I had tried many experiments on the human body, in ſucceſsfully curing paralitic, rheumatic, and other diſorders, merely by the electric powers; and obſerving their wonderful effects on the nerves in reſtoring them to a due modification, ſo as to enable them to perform their proper functions, I was led to think that our ſenſes may be performed by theſe powers; particularly, ſeeing and hearing. On my telling this gentleman my opinion, he deſired me to take care not to give that as my own, which was ſaid by another; and then brought Sir *Iſaac*'s Queries, wherein, eſpecially at the 23d query, I found, very nearly, what I had ſaid to this gentleman. But to return to my ſubject.

I need not have applied to ſo great an authority, as Sir *Iſaac Newton*'s, for the formation of vapour; ſince it is evident to the ſenſes of every man, who will enquire properly, that vapour is emitted in exceeding minute, diſtinct particles, and that it does aſcend in that form; and that above there is a colliſion of theſe particles to form larger particles, ſuch as compoſe miſts, and that they farther coaleſce 'till they come down in miſts, rain, ſnow, hail, and ſuch heavy drops as form thunder-ſhowers. When I found that all aſcending vapour was really electrified, I thought that the expanſion of that power was ſufficient to carry up the particles; and, to bring them down again, I uſed the moſt probable means I could then think of. However, there was one thing which I could not poſſibly explain; that is, how the whole quantity of electricity, which aſcended, was ever to return to the earth again. And this enquiry prevented my ſending you that letter, for at leaſt a year; 'till at laſt, tired with the ſearch, and finding the reſt of my ſyſtem very conformable to the phænomena of nature, I let it go with that known defect. And in this puzzle I might have gone on to the end of time, if I had not diſcovered, upon a farther reſearch into the nature and actions of electricity, that electricity conſiſted of two different, diſtinct, elaſtic powers, which always ſtrongly attract and condenſe each other; and, therefore, when they are equal in quantity to each other, they condenſe each other into ſo ſmall a compaſs, that they exert but little ſenſible action; but when the equality of quantity is altered by a diminution of the one power, and an increaſe of the other power, then the increaſed power has a power of expanding itſelf in proportion to the ſubtraction of the other attracting power, by which means it forms itſelf into what we call an electric atmoſphere; which atmoſphere will repel any other atmoſphere raiſed from the ſame power of electricity, but will attract any atmoſphere raiſed from the other power of electricity. Theſe atmoſpheres are always formed with an increment of rarity as they recede from the leſſened attractive power, which leſſened power always acts inward to the body which is electrified. Theſe powers I have before called the vitreous and the reſinous, only for diſtinction, as the glaſs generally throws out the firſt, and wax the ſecond; not but that I can raiſe either of the powers from the glaſs, or from the wax; nor do I know of any different eſſential characteriſtic to diſtinguiſh them by. Theſe powers are equally and ſtrongly attracted by all matter; and are always found adheſive to all matter, nearly in proportion to the ſpecific gravity of the matter, as far as I have been able to judge. Nor is it eaſy to ſubtract any part of either of theſe powers from any part of matter, without adding an equal portion of the other power; which addition it will retain, until it is ſupplied with an equal quantity of the power firſt ſubtracted. So that when any body is electrified with the vitreous power, and you take a ſpark from it with your finger, there is juſt as much of the reſinous power paſſes from your finger into the body, as there is of the vitreous paſſing from the body into your finger.'

Now, the general law by which theſe powers act on each other is this; that a vitreous atmoſphere repels another vitreous atmoſphere, with a force exactly equal to that with which it attracts a reſinous atmoſphere of equal denſity with the repelled vitreous atmoſphere: and ſo, *vice verſa*, of the reſinous atmoſphere.

And, the attracting and repelling atmoſphere (of either kind) acts with a force proportional to, and encreaſing with, its own denſity: That is, the farther a body is immerged into an electric atmoſphere, the greater force it is attracted with; as all electric atmoſpheres have an increment of rarity outward from the electrified body. But, for a farther explanation of theſe laws, and experiments to prove them, I muſt beg leave to refer to my letter to Doctor *Birch*, of the 14th of *February*, 1758.

From a great number of experiments which I have made, I find that the clouds are electrified, ſometimes with the one power, and ſometimes with the other power of electricity; and that the change is often very ſudden; for one cloud ſhall paſs ſtrongly electrified with the vitreous power, and another ſhall approach, in three or four minutes, as ſtrongly electrified with the reſinous power.

But I never had an opportunity of obſerving that this change happens ſo often as Mr. *Canton* ſays it does, ſometimes five or ſix times in half an hour. And I hope this ingenious gentleman will excuſe my telling where his miſtake may ariſe from his manner of trying this experiment. His method is with two very ſmall cork-balls, ſuſpended from the end of a wire by linen threads; and then, (when they are electrified) by bringing rubbed ſealing-wax, or glaſs under them, he thinks they are electrified poſitively, or negatively, according to the attraction, or repulſion of the wax or glaſs. But, in this, he is much miſtaken, by following the doctrine of poſitive and negative electricity, &c. and for want of knowing the real action of the different powers of electricity upon each other. For, let him fix his wire to a large body, ſuſpended by ſilk, and let that body be electrified by wax 'till the balls repel each other; then let him bring wax, well rubbed, under the balls, and they ſhall be attracted to the wax, inſtead of being repelled; or, if the wax is very lightly rubbed, the balls ſhall be repelled from it. The reaſon of their being attracted is, that the wax being more electrified than the body, the reſinous power of the wax repels the reſinous power of the balls up the linen thread, and attracts the vitreous power of the body into the balls, which are then electrified by the vitreous power, and, conſequently, attracted by the wax. In the ſecond caſe, where the wax is but little electrified, it repels the balls, without forcing their reſinous power up the threads. But, the way to try this experiment with more certainty, would be to ſuſpend a piece of bog-down by ſilk, a foot long, from the end of a ſtick; and then, touching the down with the end of a long wire, the down will be electrified; and then you may know by which power, by the rubbed wax, or glaſs; as the powers cannot change place ſo eaſily through the ſilk, as they do through the threads. There is another great miſtake to be corrected; for, he ſeems to think that the balls are electrified with the ſame power with which the clouds are electrified; but the balls are always electrified with the contrary power, which is drawn up from the earth through him by the oppoſite power of the clouds. And this may be eaſily tried, by placing a long iron rod, with needles at top, in a bottle to be charged by the clouds; and he will conſtantly find the inſide or top of the bottle, ſo charged, electrified with the power contrary to that with which the balls, or down, is electrified. But the needles at the top of the rod will be electrified by the ſame power which electrifies the down and balls; becauſe that power is there drawing off from the inſide of the bottle, in exchange for the contrary power, which comes into the bottle, while the bottle is charging. I may here appear prolix; but much more is neceſſary to explain this matter fully; which, for the preſent, I ſhall poſtpone. It would be a great matter, if we could diſcover how theſe clouds come to be electrified with the different powers; which certainly muſt happen in their firſt emiſſion from the maſſes of which they are compoſed. The purſuit of this enquiry I would earneſtly recommend.

Since it is now evident that the clouds are often electrified with the different powers of electricity, and that ſuch clouds are often in near approach to each other; and ſince I have diſcovered that electricity conſiſts of two different diſtinct powers, it is eaſy from the laws which I have given, for the action of theſe powers on each other, fully to explain the cauſe of thunder; and to ſhew how the particles of vapour coaleſce to form ſtill larger particles, till they come down in miſts, rain, ſnow, hail, and heavy ſhowers; and bring down their whole quantity of electricity with them, which before buoyed them up. But before I proceed, I beg leave to mention one experiment which I made, to ſhew the amazing expanſion of thoſe powers, and through what an incredible ſpace they will diffuſe themſelves by their own elaſtic power, when the attraction of the other power is taken off, by leſſening that power as far as we can by the electrical apparatus, which is far from an entire ſeparation of theſe powers; and the aſtoniſhing retraction and condenſation of that atmoſphere, when the powers are again rendered equal in quantity to each other. I ſuſpended a tube of ſheet lead, ſeven feet long, and near three inches diameter, in ſilk lines; I then placed a piece of bog-down on the end of a wire, on a horizontal level with the tube, and pointing towards it; I charged a phial coated with lead, and twelve cubic inches of water in the phial. Upon my holding the phial in my hand, and giving a ſpark to the tube, I found that the atmoſphere of the tube moved ſome of the fibres of the down, by attraction, at the diſtance of more than ſeven feet; an aſſiſtant then took off the atmoſphere of the tube, by touching it with his hand. And thus I went on for more than an hundred ſparks, when the down

was

was moved at the diſtance of eighteen inches. So that, meaſuring theſe atmoſpheres in a decreaſing ſeries from above fourteen feet to three feet diameter, I found they contained twenty millions of times the ſpace which the water in the phial took up. Yet all this electricity (according to the doctrine of poſitive and negative electricity) was lodged in the pores of the water, without the leaſt ſenſible encreaſe of the water. But here I muſt not let a known error go, for the greateſt part of theſe atmoſpheres never was in the phial; but each ſpark from the bottle added ſomething to one power in the tube, and at the ſame time took an equal quantity, from the other power in the tube, into the phial; and then the encreaſed power of the tube expanded itſelf into an atmoſphere, together with the additional quantity which it received from the phial. For had the tube been much leſs, the atmoſpheres would not have been ſo extenſive; however, the expanſion of that which was really in the bottle is amazing. For Sir *Iſaac Newton* has ſhewn that light approaches to a body within the 1-89000 part of an inch before it is reflected; and as I have endeavoured, in my letter to you of the 26th of *February*, 1760, to ſhew that the electric powers are the cauſe of reflection and refraction, it is highly probable that the two different powers are condenſed within that diſtance from the ſurface of the bottle (and I mention this experiment moſtly in regard to the reflection and refracting of light.) Now if a ſmall portion of one of theſe powers can diffuſe itſelf through a ſpace ſome millions of times larger than what the whole bottle takes up, what amazing difference muſt there be between its greateſt expanſion and condenſation; and yet the moſt extended electric atmoſphere is retracted and condenſed in an inſtant. The reaſon why the water in the bottle is never increaſed or decreaſed in charging or diſcharging the bottle, is becauſe there is always the ſame quantity of electricity in the water; for as one power goes out, an equal quantity of the other comes in.

It was known to Mr. *Franklin* and others, that there was no more electricity in the bottle when charged, than there was when diſcharged; but till I had diſcovered the action of the different powers of electricity, it was impoſſible to explain this matter by one ſingle power of electricity, or to explain any one phænomenon ariſing from electrical experiments. I have ſhewn, in my letter of the 14th of *February*, 1758, that there is ſomething more of the electric powers in a bottle highly charged, than when diſcharged; which appears from the bottle's recharging of itſelf, after it has been ſeemingly entirely diſcharged.

I come now to ſhew how that electricity which buoyed up the vapours may be again condenſed, ſo as to let the vapours fall in miſts, rain, &c. and to return with them to the earth.

denſed at the time of their joining with the flaſh; and that the atmoſphere, drawn up from the earth, is alſo then retracted again to the earth.

In the 47th vol. of Philoſ. Tranſ. page 550 and 551, Mr. *Ludolf*'s 4th obſervation: At every clap of thunder the electricity ſeemed extinct, and returned not till after thirty ſeconds, or thereabout, and ſometimes longer. Sixth Obſervation: They put a thread upon the wire, the two ends of which hung down; which ſhewed electricity, by mutually repeling each other; for at every flaſh of lightenling, they approached each other ſuddenly, as if they had been puſhed one againſt the other by force. Same vol. page 557, Abbe *Nollet*'s letter: And it even ſeemed, that the clap of thunder put a ſtop, for a very ſhort time, to the force of the electricity. Page 559, Mr. *Mylius* of *Berlin*: It was alſo continually obſerved, the effects were greateſt when the lightning was neareſt; and that, for ſome moments after the lightning, the effect ceaſed, but returned and encreaſed by degrees.

All theſe obſervations, and many more, I have made; but to give them more weight, I have troubled you with theſe quotations. But this matter is eaſily demonſtrated by the electrical apparatus. Let two cork balls be ſuſpended by linen threads from the end of a wire, and brought within the atmoſphere of an electrified conductor, they will be electrified with the power, which is contrary to that which electrifies the conductor, and repel each other, but will be attracted towards the conductor; but when a flaſh is taken from the conductor, the balls inſtantly fall together; that electricity, which is drawn into them from your body, by the atmoſphere of the conductor, being again retracted into your body.

It often happens that clouds are highly electrified and attract each other, and that their particles attract each other into contact, and exchange powers, without that violent flaſh which conſtitutes thunder; and that the atmoſpheres of the particles are by that means condenſed, and loſe their repulſive power, and run into farther contact with each other by the general attraction of gravity, and by accidentally impinging upon each other in their fall, and thus come down in heavy ſhowers. Now when two clouds thus attract each other into one, the particles muſt attract each other into contact, and exchange powers moſt in the middle of that cloud, and have their atmoſpheres condenſed, and conſequently come down in the heavieſt drops; and, accordingly, the heavieſt part of the ſhower is always from the middle of the cloud. To prove this doctrine, I could produce a great number of experiments, which I have made on the clouds; but ſhall only mention the laſt, which was on the 29th of *October*, 1760, when the clouds were very diſtinct, and the ſhowers heavy. In three different clouds, I found the beginning of each cloud electrified with the vitreous power; in the middle of each ſhower no ſign of electricity; and, in the end of each cloud, I found the reſinous power of electricity. Wind, N. W. The reaſon why there was not any appearance of electricity in the middle of the ſhowers, was becauſe the electric powers were equal to each other in every drop, and therefore their atmoſpheres were condenſed into inſenſible ones.

It often happens that rain, hail, and ſnow do exhibit ſigns of being electrified; becauſe the clouds are ſeldom ſo equally electrified with the different powers of electricity, as, upon meeting, to render the powers of electricity equal in each deſcending drop. And this is moſt evident in large flakes of ſnow; for when they come very near a non-electric body, they are drawn to it, and cling into it, in the ſame manner that an electrified feather does.

Now, gentlemen, I believe I need not trouble you with a prolix detail, to ſhew how clouds differently electrified with the different powers may approach each other; or how the particular particles may attract and run into each other, and have their atmoſpheres condenſed according to the different degrees in which they were electrified by the different powers; ſince the general laws, which I have given for the action of the electric powers, will explain all the phænomena ariſing from electrical experiments, which I know of, at leaſt I never found them deficient in any experiment which I could think of making;

whether I reaſoned from the laws to the effects, or from the effects to the laws. And, therefore, I think I may claim the honour of having diſcovered this new doctrine of electricity, without the knowledge of which, I think it is impoſſible fully to explain any one phænomenon ariſing from electrical experiments. But if any ingenuous gentleman can ſhew me where thoſe laws or properties of electricity are wrong, I ſhall be really obliged to him; for I am much more deſirous to diſcover truth, than to appear as an author.

And now, gentlemen, I hope that you will excuſe my pointing out ſome of the abſurdities of Mr. *Darwin*'s theory, which contradict the reaſon and experience of all mankind. He firſt ſuppoſes that water is ſo far expanded by heat as to riſe in the atmoſphere by the laws of ſpecific gravity; and then that it is better ſupported in a rarer and colder part of the atmoſphere above, than in a warmer and denſer part of the atmoſphere below; he ſays, becauſe the preſſure of the atmoſphere is leſs above, and for that reaſon the water can better expand itſelf. If vapour ariſes by the laws of ſpecific gravity, it muſt aſcend quickeſt where the preſſure of the atmoſphere is greateſt, the expanſion of the vapour remaining the ſame. And it is impoſſible to ſuppoſe that the expanſion of vapour is encreaſed by heat above, where vapours are ever found colder than the circumambient air, and the circumambient air there always colder than that below. This is a fact well known to every man, that had his feeling, who ever paſſed over high mountains. He ſays that he is perſuaded, that was it not for the preſſure of the ſuperincumbent atmoſphere, greatly leſs than that of boiling water would inſtantly diſperſe the whole ſo heated into vapour. But would this gentleman have us ſuppoſe that this vapour is to ariſe by the laws of ſpecific gravity, where there is not any medium ſpecifically heavier for it to riſe in? When the impulſive or repulſive force ceaſes, which diſperſed it, muſt not this vapour return inſtantly to the earth by the general laws of gravity? Does this any way point out the cauſe of the aſcent of vapour, or ſhew us how the clouds float by the laws of ſpecific gravity, which they evidently do? Indeed he has found out one extraordinary way of ſupporting them, which is by the motion they acquire from the winds. This might do, if the wind blew from the Nadir to the Zenith; but as it generally blows on a horizontal level, I think it may as well blow them down as up. He then quotes the opinion of Sir *Iſaac Newton*, to ſhew that bodies are only heated by reflection and refraction of the ſun beams. How he came to imagine that this was Sir *Iſaac*'s opinion, I can't conjecture; for Sir *Iſaac*'s eighth propoſition, of his ſecond book of Opticks, ſtands thus: "The cauſe of reflection is not the impinging of light on the ſolid or impervious parts of bodies, as is commonly believed;" and this Sir *Iſaac* has fully proved, both as to reflection and refraction; and all experience ſhews that black opake bodies, which produce but little reflection or refraction, are exceedingly more heated by the ſun, than diaphonous bodies, where the reflection and refraction are exceedingly great. Place a convexo convex-glaſs oppoſite the ſun, and at the ſame time a black body of the ſame magnitude, and you will find the one grow very hot, while the other retains but very little heat.

This gentleman's chief principle is, that the ſun heats vapour, but does not heat the air: But, leſt you may think I miſtake him, pleaſe to hear his own words.

" The ingenious Mr. *Eeles*, I dare ſay, has " already foreſeen the uſe I am going to make " of this principle; viz. That the little ſphe- " rules of vapour will thus, by refracting " the ſolar rays, acquire a conſtant heat, tho' " the ſurrounding atmoſphere remain cold."

I think that this gentleman ſhould have produced ſome better proof of this matter, before he endeavoured to make us diſcredit our ſenſes. Does not every man know the air is warmer at one time than at another, and that it is ſometimes very warm? If the ſun does not warm the air, I deſire to know what does warm it? As to his aſſertion, that clouds are warmer than the circumambient air, I declare that it is entirely falſe; and I appeal to the experience of every man, who ever paſſed through them on the ſides or tops of high mountains, whether he has not conſtantly found them to be damp, chilling fogs, much colder than the circumambient air; and ſometimes the particles ſo large as to wet a man conſiderably; tho' in paſſing from one mountain to another they do not let any of their wet fall into the valley between. All this I have experienced a thouſand times; therefore Mr. *Darwin* muſt excuſe me for chuſing to be guided by the evidence of my own ſenſes, rather than by his aſſertion.

But if I muſt produce an authority for what is well known to ſo many thouſands, pleaſe ſee Dr. *Peyſoneld*'s account of the *Brimſtone-hill*; 49th vol. Phil. Tranſ. page 571: "Beſides the fine proſpect you enjoy at the top "of this mountain, you have the pleaſure, as "father *Du Tertre* obſerves, of ſeeing the "clouds gather below, and hearing the thunder "rumble under your feet. We actually "ſaw the clouds riſe from the ſea, and ſpread "over the land on the ſide of the wind, and "ſometimes paſſing where we ſtood, and ſometimes "lower. Theſe clouds are no other "than damp fogs. The *Brimſtone-hill* is ſeldom "clear of theſe damp fogs." And in the preceding paragraph he tells you how the naked negroes often have their blood chilled by theſe damp fogs, ſo as to periſh by it.

Mr. *Darwin* ſays, from conſidering this power of expanſion, which the conſtituent parts of ſome bodies acquire by heat; and withal, that ſome bodies have a greater affinity to heat; that is, acquire it ſooner, and retain it longer, than others; that many things, before utterly inexplicable, became eaſily underſtood by him; particularly how that ponderous body, mercury, may be raiſed into vapour. But, unluckily, he has not told us how theſe matters became eaſily underſtood by him, or how they may be eaſily underſtood by us. I muſt confeſs, that if he could explain how the particles of mercury may float upon the air, that then he may eaſily explain how the particles of water may float there; but till he has done one or the other, I ſhall leave his theory to be farther conſidered by thoſe who think it worth the while.

The deſign of this gentleman's ſecond letter is, according to his own words, entirely to deſtroy the probability of my notions; and to give an additional ſupport to his own theory. And then he ſays, "For this purpoſe, " our firſt endeavour will be to ſhew the un-" certainty of ſome of the moſt material prin-" ciples that ſupport his arguments; and af-" terwards the fallacy of the experiments he " has given us.

" Firſt, then, in page 130, Mr. *Eeles* has " aſſerted, that the greateſt poſſible rarefac-" tion of water is when it boils."

This he has taken from a curſory obſervation of mine, which ſtands in theſe words, and which I think may ſtill keep their place. " And here I muſt obſerve, that it has not " yet been explained how water can be dila-" ted, ſo as to occupy eight hundred, or a " thouſand times the ſpace, which it naturally " does. For I think, the greateſt expanſion " it ſuffers by boiling (the greateſt heat we

" can give it) is not more than a nineteenth " or twentieth part of what it was before."

This gentleman accuſes me of a confuſion in terms, while himſelf has bred the confuſion; for he has changed my word, dilated, into the greateſt poſſible rarefaction: By dilatation, I meant that expanſion which bodies ſuffer, without any ſeeming diſcontinuity of their parts: Rarefaction, I know, is often applied to the dividing a body into innumerable parts. He then goes on thus.

" In page 133, the ſphere of electrical activity is ſaid to be encreaſed by heat. If by " electrical activity is here meant an encreaſe " of its repulſive power (the thing which " ſeems to be wanted in Mr. *Eeles*'s hypotheſis) I know no experiment to ſhew it."

I can eaſily grant that this gentleman may be ignorant of experiments to prove this and many other matters that are neceſſary to a moderate underſtanding of electricity; but if he wanted eyes, would he, for that reaſon, perſuade us that there was not any light? If it be required, I can produce experiments which fully prove that the electrical powers are expanded by heat: A clear knowledge of which is very neceſſary to a right underſtanding of the action of theſe powers in many of the operations of nature, and many of thoſe of art, where natural powers are made uſe of. In the next paragraph he endeavours to perſuade us, that electricity does not act upon the air; tho', three lines before, he ſays, that he knows that the electric æther is attracted at very great diſtances in *vacuo*. Tho' many experiments do prove that electricity does act upon the air, I ſhall refer him to Mr. *William Watſon*'s experiments in *vacuo*, in the 47th vol. of Philoſ. Tranſ. page 362, where he has taken a great deal of pains to ſhew that air does act upon electricity, in confining it to non-electric bodies. If this be true, I think we may boldly affirm that electricity re-acts upon the air. And I have not any reaſon to doubt Mr. *Watſon*'s doctrine (as far as it relates to the preſſure of the air upon an electric atmoſphere) but that he now recommends Mr. *Darwin*'s doctrine, which is directly contrary; which may be from ſome new diſcoveries which I am not acquainted with.

To illuſtrate that air is not acted upon by electricity, Mr. *Darwin* produces an experiment with a long-necked bottle ſuſpended by ſilk, with the neck downwards, immerged in oil of turpentine, ſupported by an electric; and the inſide of the bottle lined with gilt paper; and then warmed ſo, that, when cold, the oil was forced up half way the neck. Then with a bent wire he electrified the inſide of the bottle; and finding that the oil was not forced out of the bottle, he concludes that the electric atmoſphere did not act upon the air. If this gentleman had conſidered the force neceſſary to move this column of oil, which force muſt be able to ſupport a column of the ſame height, and a baſe equal to the whole ſurface of the inſide of the bottle; and that air is an elaſtic body, and that the electric atmoſphere is exceedingly ſo, perhaps his ſurprize may leſſen. And if he will pleaſe to add to this what I have ſaid in regard to charging the *Leyden* bottle, that as one power of electricity goes into the bottle, an equal quantity of the other power comes out; and to conſider how minute a part of that power, which predominates in the bottle, is expanded into an atmoſphere, he will ſcarcely think the force of this atmoſphere equal to the force which I have ſaid was neceſſary to drive the oil out of the bottle. Add to all this, that this atmoſphere has a power of paſſing thro' the glaſs; which he may eaſily try, by touching the outſide of the bottle with a piece of bog-down ſuſpended by ſilk, and he will find the outſide of the bottle as much electrified as the inſide, and with the ſame power of electricity. I could now produce an experiment which would nearly aſcertain how much an electric atmoſphere preſſes upon the air in ſuch a bottle; but that I fear I may tire your patience.

In the next paragraph Mr. *Darwin* tells us, that he had formerly tried this experiment various ways, in order to make the electric matter anſwer the end of ſteam in the ſteam-engine, and many other great mechanical purpoſes. In this I think he has given us a full proof of a ſtrong inventive faculty, and a clear unbiaſſed judgment (which, in the beginning of his letter, he ſays, are neceſſary for making experiments) for I dare ſay that he did not ſteal this deſign from any man living; to apply a power to great mechanical purpoſes, without knowing any thing of the force which that power acted with, or its manner of acting. And yet I can tell Mr. *Darwin*, that the buſineſs of the ſteam-engine is moſtly carried on by the electric powers; but ſhall not now ſtop to explain this matter. If Mr. *Darwin* has a mind to enquire how theſe ſubtile parts of matter act on each other, I would deſire him to enquire how it comes to paſs, that in a room filled with air, the rays of light can croſs each other in ſo many millions of angles, as is neceſſary to give the eye a power of ſeeing any part of the room, to which a right line can be drawn, let the eye be moved as often as you pleaſe from its firſt ſtation; next, how all the operations of electricity can be carried on in the ſame room; alſo, all the operations of magnetiſm; the operations of culinary heat, and all the effects of gravity, by which every particle of the whole is affected; and yet theſe powers give but little obſtruction to each other, which can be made evident to our ſenſes. When Mr. *Darwin* has explained theſe things, it may help me to explain his experiment.

Mr. *Darwin* now proceeds to ſhew you the fallacy of my experiments; but, unluckily, as far as he has gone, he has proved the truth of them; viz. that aſcending vapour does paſs by electrified bodies, without diveſting them of that power which electrified them. But why they do ſo, is not to be underſtood without a perfect knowledge of the action of the different powers of electricity. Indeed he has ſhewn a thorough ignorance in making electrical experiments, and ſubſtituted a monſtrous falſity of his own. Firſt, he ſuppoſes that the *Leyden* bottle, a dried feather, a dried cork, and the bog-down, which I have mentioned, are equally fit to try electrical experiments with; whereas a dried feather and the bog-down are as different matters for this purpoſe, as glaſs and metal. He ſays, that the *Leyden* bottle may be touched three or four times by a quick finger, before the whole is diſcharged. While the bottle was ſuſpended by ſilk, I have touched the top and bottom alternately with my finger, ſeventeen hundred times before it was diſcharged; tho' there were but twelve cubic inches of water in the bottle. A dried feather will not conduct electricity more than dried ſilk, or glaſs, and therefore, like them, when electrified, may be touched in many parts, before all the power is withdrawn, by which it was electrified. A dried cork acts ſomething in the ſame manner; but the bog-down conducts electricity nearly as quick as metal; and, therefore, when electrified, cannot be touched by the finger, in any part, for half a ſecond before all ſigns of its being electrified diſappear. A piece of this down, ſuſpended by ſilk, againſt the ſide of a conductor, will play between the finger and conductor, while the wheel is turning, ſo quick, that the ſilk, by which it is ſuſpended, will appear like a narrow ribbon; and it is evident, that while it moves to the conductor, it is electrified with the one power; and when it moves from the conductor, it is electrified with the other power of electricity. Yet has Mr. *Darwin* the aſſurance to tell the Royal Society, that this down may be touched at intervals, for half a minute, by the finger, or any known conductor of electricity, before it would part with its electricity. This experiment is ſo very eaſily made, that the Royal Society cannot long doubt which of us has told them an untruth. Mr. *Darwin*'s words ſtand thus: "From whence I apprehend, that Mr. *Eeles*, "having dipped the electrified down of the "juncus bombicinus in vapour, for perhaps "half a minute (for no time is mentioned) "and finding it ſtill retained its electric at- "traction, was not aware that this ſame had "happened, if he had by intervals touched "it with his finger, or any other known con- "ductor of electricity."

In my letter, where I have mentioned this matter, my words ſtand thus: "But to try "whether the ſteams, &c. were non-electrics, "I only bedewed the wax and glaſs with my "breath, ſteams, &c. from my hand to the "end of the wax and glaſs; and then touch- "ing the electrified down with the end of the "wax or glaſs, I found that the electrical fire "immediately paſſed from the down into my "hand, through the ſteams, &c. which reſt- "ed upon the wax and glaſs."

Now whether this ſtrange apprehenſion of Mr. *Darwin*'s (where he apprehends that I did not do, what I expreſsly ſaid I did do) and that the effects of an experiment (which I can't believe he ever made) would be contrary to what I have declared they were, and to what, I now aſſert, they always will be, is ſufficient to ſhew the fallacy of my experiments, and to overturn all I have ſaid about the cauſe of the aſcent of vapour, &c. I muſt leave to the Royal Society to determine.

Next, Mr. *Darwin* takes a great deal of pains to prove what every body knew, and what I never contradicted; but, on the contrary, poſitively aſſerted; viz. that light bodies electrified by glaſs were attracted by electrified wax, and ſo *per contra*: what I ſaid upon that occaſion was expreſsly a hint to enquire, whether electricity was not differently actuated by different qualities in the wax and glaſs? which I have ſince found to be true; and alſo, that electricity itſelf conſiſted of two diſtinct different qualities or powers; which I have proved by numberleſs experiments, without ever meeting any one experiment to contradict it. From which, and many obſervations thereon, I have been enabled to apply the electric powers to medicinal uſe, from a reaſoning *a priori*, which I think nobody elſe has yet attempted. And I have performed ſome cures, which the phyſicians here thought very little leſs than miraculous; and indeed I thought ſo too, the electrical powers having perform'd more than I could poſſibly expect in many caſes. And to give you an idea of what they will do, when properly applied, I beg leave to mention what was ſaid by a patient of mine, who had been with me for ſome time.

Mr. *Thomas Gard*, who was univerſally paralitic, except his head, and who was bred an apothecary and chymiſt, has enquired of me, with ſurprize, in the following terms; viz. Dear Sir, how is it poſſible for you to tell me beforehand what you intend to do; and then, in a few minutes, to give my limbs a power of what motions you pleaſe; and that you can retract that power when you pleaſe, and give them a power of other motions as you think proper; that they ſeem no longer obedient to my will, but to yours.

I had drawn up an account of ſome of theſe cures to lay before the Royal Society, not ſo much from a vanity to tell what I had done, as to caution the unſkilful not to attempt to uſe the electrical powers in a medicinal way; for great miſchief may be done by them. I ſhall here only mention that three things are neceſſary to any attempt of this kind; which do not meet in every man who has an electrical apparatus. A ſkill in medicine, a ſkill in anatomy, and an accurate ſkill in electricity and the manner of conducting its powers.

In the next paragraph Mr. *Darwin* quite triumphs over me. " But what alone would " entirely deſtroy this electric hypotheſis is, " that, from the experiments of Mr. *Franklin* " and others, the clouds are ſometimes found " to be electrified plus, ſometimes minus, and " ſometimes manifeſt no ſigns of electricity " at all."

Altho' I do not know that any body beſides Mr. *Canton* has taken notice that the clouds are frequently electrified by the different powers of electricity, which he calls poſitive and negative; yet I can eaſily grant that they are ſo, becauſe I know it to be true from a great number of experiments which I have made. But, pray how does this entirely deſtroy my hypotheſis? Does it not confirm it as much as is poſſible, as I have ſhewn in the courſe of this letter? His laſt aſſertion, that ſometimes the clouds manifeſt no ſigns of electricity at all, may be true, as to any experiments which can be made by Mr. *Franklin*, or by Mr. any body elſe, on a light cloud, at a thouſand or two thouſand yards diſtance; and yet the particles of that cloud may be highly electrified, though their atmoſpheres are not preſſed off to this diſtance.

Here Mr. *Darwin* has again been unlucky, and has proved the thing which he wanted to diſprove.

And what is more, almoſt the only thing which was wanting to compleat my ſyſtem: For had I known at the time of writing my letter, that different clouds were frequently electrified with the different powers of electricity; and had I known how theſe powers acted on each other, it would have ſaved me a great deal of thought and contrivance, to ſhew how the electrified vapour may again return to the earth; as any body may ſee, who does me the honour to read my letter with attention; and would eaſily have proved how the whole quantity of electricity, which aſcended, did again return to the earth; which, at that time, was impoſſible for me to prove, as I began late in enquiries about electricity; for at the time of writing you an hypotheſis, to ſhew the cauſe of thunder, I had never read any writer upon electricity; nor had I any other knowledge of it, than what was furniſhed by my own obſervations upon ſome experiments, which were exhibited for two nights ſucceſſively as a ſhew in the year 1748; and in the year 1750, accidentally thinking that the cauſe of thunder was not ſufficiently explained, I recollected that the electric fire ſubſiſted in water; and finding the ſeat of thunder was alſo in water, I began to think how the electric fire may be the cauſe of thunder; and from thence drew up that hypotheſis, which, tho' entirely my own from that ſlight aſſiſtance, I may ſay, is as full and clear (as far as electricity was then underſtood) as any thing that I have met with, which has been produced upon that occaſion.

And as that diſcovery was as much my own invention, as if an analogy between thunder and electricity had never been thought of by any body elſe; ſo without any other aſſiſtance, than my own experiments and obſervations, I have purſued the explanation of the cauſe as near to a demonſtration, as the nature of the proofs will admit. It is true that I have read ſome tracts upon electricity; but as they came late to my hands, I can't ſay that they gave me any aſſiſtance in my enquiries; on the contrary, I muſt ſay, that if I had reſted in any of the opinions which I met with, I muſt have ſtopped ſhort of the truth.

The other part of Mr. *Darwin*'s aforementioned paragraph ends ſo forcibly, and with ſo polite a compliment to me, that I cannot forbear troubling you with it.

'Whence to ſay an accumulation of electric 'æther ſupports theſe clouds, ſeems an aſſer'tion built upon a very unſtable foundation, 'whoſe whole ſuperſtructure may well enough 'be termed an air-built caſtle, the baſeleſs 'fabric of a viſion.'

I muſt confeſs, gentlemen, that I have built my clouds upon the air, and every body ſees that the foundation is ſufficient; but Mr. *Darwin* has vaſtly outdone me, for he has built without any foundation at all. I am,

GENTLEMEN,
your moſt humble and
moſt obedient ſervant,
HEN. EELES.

I have inſenſibly been drawn into a prolixity by anſwering the abſurdity of Mr. *Darwin*'s theory, and remarks upon what I had wrote; for which I am conſcious that I ought to aſk your pardon. My chief deſign was only to give a farther explanation of the cauſe of the aſcent and deſcent of vapour and exhalation, &c. and to ſhew that they aſcend and deſcend by the action of the ſame powers; and that the powers entire may both aſcend and deſcend with them; which never can be explained by the doctrine of poſitive and negative electricity, or a plus and minus of the ſame power. So that my doctrine of the two powers of electricity muſt be accepted, or electricity be excluded from the buſineſs of the clouds; which cannot well be allowed againſt the evidence of ſo many well-known experiments.

⁂ Sent this letter, dated 20th *March*, 1761, to the Royal Society.

LETTER IX.

SIR, *Liſmore*, 26th *Feb.* 1760.

SINCE my letter of the 14th *February*, 1758, I have made many electrical experiments; all confirming the doctrine and laws which I mentioned in that letter. As ſome of them are quite inexplicable by any thing which was ſaid of electricity before that time, or indeed by any thing I have ſince met with, I ſhall mention two or three of them. Let a conductor be electrified, either by a glaſs globe or a globe of ſealing-wax: I can, in a few ſeconds of time, with the atmoſphere of that conductor, electrify a large plate of glaſs, or a thin plate of ſealing-wax, ſtrongly on both ſides with the vitreous power;

er; and then withdraw the vitreous power, and leave both ſides electrified with the reſinous power; or then can withdraw that power from one ſide, and leave the different ſides electrified with the different powers; and all this without any contact with the conductor; and the plates ſhall remain ſo electrified, in any of the aforementioned ſtates, when carried quite out of the atmoſphere of the conductor: Or I can charge Mr. *Muſchenbroek*'s bottle, ſuſpended by a dry ſilk cord in the atmoſphere of that conductor, ſeveral inches from the conductor, and without any contact with that, or any other matter but the ſilk cord, firſt with the vitreous power inſide, and the reſinous power outſide; and then withdraw that charge, and recharge the bottle with the powers in the contrary order; without the bottle's having any communication with any thing but the ſilk cord. I ſhall take another opportunity of explaining this experiment, and of ſhewing that the electricity found in the air during the time of thunder, &c. does not only come down from the clouds, but is alſo drawn up from the earth by the electric atmoſphere of the clouds. I can, with an electrified glaſs, electrify a piece of down through a glaſs which is not electrified; and that with either of the electric powers, as any ſpectator ſhall pleaſe to direct at the inſtant of performance: Or I can do the ſame thing with an electrified ſtick of ſealing-wax: Or, with the ſame piece of wax, I can electrify a piece of bog-down, ſuſpended by ſilk, with either of the electric powers; and then withdraw that power, and electrify it with the contrary power; and ſo on for ſeveral times in ſucceſſion; and yet a ſpectator ſhall think that I touch the down with the wax in the ſame manner to give both the powers.

I would now aſk how theſe experiments are to be explained by the doctrine of poſitive and negative electricity? or that glaſs throws out the electricity, and that wax drinks it in? or the ſuppoſition that glaſs is impermeable by electricity? or the wanting and abounding ſides of the glaſs in the charged bottle, as Mr. *Franklin* has explained it; that one ſide of the bottle parts with exactly the ſame quantity of electricity which the other receives; and, on forming a communication between the inſide and outſide, that the emptied ſide greedily drinks up the ſuperfluity of the abounding ſide. But I will not detain you with arguments of this kind.

Thus you ſee I have called theſe powers vitreous and reſinous, merely for diſtinction's ſake; for I do not know of any different eſſential characteriſtic to diſtinguiſh them by. I had ſome regard to their being ſo called by gentlemen, who fancied that they found them exiſting ſingly, the one in glaſs, and the other in wax, &c. For Mr. *Muſchenbroek* ſays, that nobody can be in poſſeſſion of both powers at the ſame time: in which he is ſo far miſtaken, that theſe powers can never be intirely ſeparated in any body, or brought to any thing like an intire ſeparation.

I have ventured to try various experiments on the human body with theſe electrical powers; by which I have introduced them into medicinal uſe, and have performed many ſurprizing cures in paralitic, rheumatic, and many other caſes; and have done ſome things with them which were not thought to be in the power of the *materia medica*. An account of which I will lay before the Society, if they think it acceptable.

In my letter of the 9th of *Auguſt*, 1757, I mentioned a great ſimilarity between the action of the electric and magnetic powers; on more mature examination I find, that the laws which I have given, in my letter of the 14th of *February*, 1758, for the action of the electric powers on each other, will exactly ſerve for the action of the magnetic powers on each other: Though, to a perſon not well verſed in theſe matters, there may appear a ſeeming difference, which, upon examination, will be found to ariſe merely from the different poſition in which the different powers in the magnet are placed, from that in which the different powers of electricity are placed; for in electricity the powers change place from one body to another, which in magnetiſm they do not. And this may prevent the troubling you with the recital of thoſe laws; or if you think fit to be troubled with it, I can recite thoſe laws in a clearer and more connected manner than I have yet done.

Herewith I ſend you a letter to the Royal Society, ſhewing what I think is the cauſe of the reflection and refraction of light.

It is impoſſible for me to foreſee all the objections that will be made to it; or if I were to anſwer beforehand all the objections which I think will be made to it, I muſt enter upon a long tract. But as Sir *Iſaac Newton* is the great maſter in the doctrine of light, I think it will be ſufficient to ſhew, that the cauſes, which he has pointed at, are not ſufficient for the purpoſe. He has mentioned air; the thickneſs of the tranſparent bodies; the action of bodies upon light, at a diſtance; (by which, I ſuppoſe, he means an attraction) and his ſubtile medium, evenly diffuſed over the ſurfaces of bodies.

As to air, he has ſhewn that it is not the cauſe; and, if he had not, there is a ſelf-evident reaſon againſt it; becauſe refraction is performed between the confines of air and all other bodies. The thickneſs of tranſparent plates cannot, in itſelf, be the cauſe; for he has ſhewn that reflected and refracted light does not impinge on bodies; indeed the thicker tranſparent plates are, the more the refractive power will adhere to them. As to the action of bodies upon light at a diſtance, (which Sir *Iſaac* ſeems to ſtick to; for, in his fifth query, at the end of his book of optics, he aſks, do not bodies act upon light, in reflecting, refracting, and inflecting of it) I think it cannot be the cauſe; for when a ray of light has paſſed through a tranſparent body, if that ray be again drawn back into that body by attraction, that attraction muſt be ſtrong enough to hold that ray affixed to that body, as long as the attractive power remains the ſame; and therefore there never could be any reflection back again through a tranſparent body; but all experience ſhews ſuch a reflection to be common. As to a ſubtile medium, evenly diffuſed over the ſurfaces of bodies, it certainly muſt be the cauſe; and had he reſted it there, he would have been very near the truth. But when he had a mind to apply it to other purpoſes, and to make it the cauſe of gravity, he propoſed his æther of ſuch a form as would not anſwer either of his purpoſes: for he ſuppoſes it to be a ſubtile elaſtic medium, with an increment of denſity from the ſun through all the planetary regions, preſſing the planets from the denſer into the rarer parts of this medium; and thus acting as a centripetal power to keep them in their orbits. But I muſt here obſerve that this æther, as a centripetal power, could only act on the planet with a force proportional to the increment of its denſity, in the length of the diameter of the planet; but it muſt obſtruct the progreſſive motion of the planet with a force equal to its whole increment of denſity, from the ſun to the center of the planet, and in theſe circumſtances can never be the cauſe of gravity; and for many more reaſons which

I could add; one of which is, that gravity is performed by one ſingle power, which all bodies, and all parts of bodies, have of attracting each other; which can never ariſe from ſuch an æther. As to ſuch an æther's being the cauſe of reflection and refraction, &c. how can we ſuppoſe it? for if the reflecting medium can turn back the rays of light in the 89,000th part of an inch, as Sir *Iſaac* ſays it does, how is it poſſible for light ever to arrive to us from the ſun through ſuch a medium?

I deſire that what I have ſaid here may be looked on as a ſincere ſearch after truth, and not as a reflection on the character of ſo great a man as Sir *Iſaac* certainly was; for no man has a higher eſteem for his memory than I have; and this matter he himſelf has left and recommended to our enquiry; and I muſt obſerve, that it was impoſſible for him, or any man at that time, to know what a ſubtile medium, and how fitted for the purpoſe of refraction, &c. did really ſupervade the ſurfaces of all bodies. Nor is it now eaſy to conceive, but by thoſe intimately acquainted with the doctrine which I have given of the electric powers; and by ſuch as have gone through a great number of experiments, to convince themſelves of the truth of that doctrine. Sir *Iſaac Newton*, in ſpeaking of his ſecundary attraction and repulſion at very minute diſtances, makes a great gueſs at the uſe of electricity in that caſe; for he expreſsly ſays, who knows but the power of electricity may extend itſelf to theſe minute diſtances without being excited by friction, which is certainly the caſe. And here that attraction and repulſion may be eaſily ſhewn to be the effect of the electric powers: in the attraction the force of gravity muſt alſo act. But I believe I had better explain this matter in a paper by itſelf. Had he then known as much of electricity as we now do, and that the electric powers adhered to all bodies, I am certain that he would have made a better uſe of that knowledge than I am able to do. However, I am confident that in time the electric powers will be fully proved to be the cauſe of the reflection and refraction of light.

I ſhall be very much obliged to you if you will let me know the moſt material objections which ſhall be made to what I write to the Society. I am,

SIR,

Your moſt humble and
moſt obedient ſervant,

HENRY EELES.

To the Rev. *Thos. Birch*,
Secretary to the Royal
Society.

LETTER X.

Liſmore, 26th *February*, 1760.

GENTLEMEN,

IN the year 1752, I promiſed you to ſhew that the fire or fluid of electricity was the cauſe of the refraction, &c. of light; and accordingly wrote down my reaſons, which, on farther conſideration, did not appear ſatisfactory to me, and therefore did not trouble you with them; nor was it poſſible then to explain this matter, the action of the electric powers not being at all underſtood. But ſince I have diſcovered that all the phænomena of electricity ariſe from the action of two different diſtinct powers, which always act conjointly, and explained their manner of acting on each other; (which I communicated to Doctor *Birch* in two letters, of the 9th of *Auguſt*, 1757, and 14th of *February*, 1758) it appears plainly to me, that theſe powers are the cauſe of the reflection and refraction of light.

The great difficulty of explaining refraction was, to ſhew why the rays inclined to the perpendicular, in paſſing from a rare into a denſe medium; and receded from the perpendicular, in paſſing from the denſe medium into a rarer one. I will not detain you to ſhew the inſufficiency of all the reaſons aſſigned, but muſt beg leave to object to the principal one; which was, ſuppoſing that the refracting medium had a vibrating motion which accelerated the rays in their paſſage through it; which, I think, muſt be wrong.

For firſt, how is it poſſible that any body can be accelerated by paſſing through any medium? Muſt not the body meet a reſiſtance in proportion to the depth and denſity of the medium? If the medium moves faſter than the body, how can the body overtake the medium to paſs through it? Is it intelligible how any body in paſſing through any medium, can put that medium into ſuch a motion as may accelerate the body? If it be ſuppoſed that the medium is naturally in a vibrating motion, to make the pulſes quicker than the motion of the body paſſing through it, its velocity muſt ſometimes be as great againſt the motion of the body, as at other times it is with it; and when reflection is cauſed by ſuch a vibrating motion, the angle of reflection muſt differ as much from the angle of incidence, as the angle of refraction does; the rays being as much accelerated in reflection, as in refraction, by ſuch a medium. Muſt not theſe vibrations incline the rays as much to the perpendicular in their paſſage out of a denſe body into a rare one, as they do when the rays paſs out of a rare body into a denſe one? But this is contrary to all experience; and it is more difficult to explain how ſuch a vibrating motion, the pulſes of which ſhall have a greater velocity than light, can be cauſed and continued, than to explain all the difficulties of reflection and refraction, by means of a ſubtile medium at reſt. Now if it can be proved that reflection and refraction may be cauſed by a denſe elaſtic medium of ſubtile parts, and that ſuch a medium does really exiſt, covering the ſurfaces of all bodies, and that in proportion to the denſity of the bodies; (for Sir *Iſaac Newton* has ſhewn that bodies have a refractive, power in proportion to their denſities) I think that the doctrine of reflection and refraction (for as for deflection or inflection, it will appear to be only refraction by the different poſition of the medium) will be made much more intelligible than it has been hitherto, and the cauſe of reflection and refraction fully proved. Sir *Iſaac Newton* has ſhewn that ſulphureous bodies have a greater refractive power than other bodies, in proportion to their denſities; it will be here alſo ſhewn, that this medium adheres to ſuch bodies in greater quantity than to other bodies of the ſame denſity.

The above great author, in his firſt book of optics, has given us a clear doctrine of the refrangibility of light, and of the ſeparation of its colours thereby; and an intire confutation of that doctrine which ſuppoſes light to be propagated by preſſion; ſo that I need not take notice of what has been ſaid by ſome gentlemen who have lately attempted to revive that doctrine. But as the cauſe of refraction, &c. was confeſſedly unknown to this great man, he only ſuppoſed that it muſt be performed by the pulſes of ſome ſubtile medium, which preſs forward the rays in refraction.

As to the manner of acting, I have before objected; and I ſhall now endeavour to ſhew how refraction may be performed by a ſubtile medium's retarding the rays of light in their paſſage through it; and to ſhew what that medium is, and the manner how it acts.

I ſhall firſt conſider the caſe of a ball ſhot through a board. Suppoſe the line of direction of the ball to make an angle of forty degrees with the plane of the board, and another plane inclined to the plane of the board in that direction, dividing the ball into hemiſpheres; it is plain that the hemiſpheres next to the board muſt firſt come in contact with it, and give the ball a direction from the perpendicular to the board, till the ball is intirely immerged; and whatever direction the ball then has, it muſt retain till it begins to immerge, and then the direction muſt be altered in the contrary order from which it is immerged, and proceed in a line parallel to the line of its firſt direction.

I will now conſider this board of another contexture, with the ſide very rare which is to be entered by the ball, and that ſide very denſe from which the ball is to immerge; the conſequence will be, that the ball ſhall not be ſo much altered in its direction from the perpendicular in immerſion, as it ſhall be altered to the perpendicular in its immerſion from the denſe ſide of the board; and therefore the angle of refraction will bear a ſimilar proportion to the angle of incidence, as it does in light paſſing from a rarer into a denſer medium. And now ſuppoſe the ball paſſing back again through this board, the angles of incidence and refraction ſhall be ſimilar to thoſe of light paſſing from a denſer medium into a rarer.

Now I think all that is to be done to explain the refraction of light, is to find a ſubtile medium that ſupervades the ſurfaces of all bodies; whoſe denſer ſide ſhall always adhere to the confines of the denſer medium or body to which it is applied, and its rarer ſide always joined to the confines of the rarer medium or body.

Whoever attentively conſiders the doctrine and laws of electricity which I have given in my letters of the 9th of *Auguſt*, 1757, and 14th *February*, 1758, muſt ſee that the electric powers muſt always adhere to all bodies in this manner; for whether the vitreous or reſinous power of electricity be encreaſed, the encreaſed power will act outward from the denſer body, with an atmoſphere with an increment of rarity outward, which will increaſe in proportion to the increaſe of the quantity of that power above the contrary leſſened as to let me know their objections, it will give me an opportunity of explaining this matter better, or of ſubmitting and confeſſing that ſome other cauſe muſt be enquired for. I am,

Your moſt humble and
moſt obedient ſervant,

Henry Eeles.

To the Royal Society.

LETTER XI.

My Lord, *Liſmore*, 29th *Dec.* 1761.

WHILE you preſide over the Royal Society, I hope that you will forgive my troubling you with a letter relative to the tranſactions of that Society. I am conſcious that it muſt appear the higheſt preſumption in any ſingle man to conteſt with the Royal Society; and if I thought my preſent expoſtulation was with the whole Society as a body, I ſhould have modeſty enough to think ſo too, and deſiſt from giving your lordſhip the trouble of this letter; but as the matter of my preſent complaint ſeems to ariſe from the unwillingneſs of a few members of the Society to underſtand ſome truths which I have produced to the Society, and that thoſe truths are of the higheſt importance toward the making new diſcoveries, and the increaſe of natural knowledge, for which end, I preſume, the Royal Society was peculiarly inſtituted, I ſhall venture to proceed.

But firſt I muſt beg leave to mention the tracts which I have laid before the Royal Society, and the ſucceſs they met with; to ſhew the reaſon why I troubled them in particular, or the world in general, with any opinions of mine; and how I have been drawn into this expoſtulation.

In the year 1750, as I was accidentally reflecting that the cauſe of thunder was not ſufficiently explained, I recollected, that in ſome electrical experiments which I had ſeen in the year 1747, that the electrical fire ſubſiſted in water; and knowing that the ſeat of thunder was in heavieſt ſhowers of rain; and imagining that there was ſome analogy between the effects of thunder and thoſe of electricity, I began to think how the electric fire may be the cauſe of thunder; and in *September*, 1751, I drew up an hypotheſis to explain that matter; and, in *June*, 1752, I ſent it to the Royal Society; and it was honoured with their acceptance, and printed in the Philoſ. Tranſ. for that year. At the end of that hypotheſis, I mentioned ſome ſubjects which I thought might be explained by electricity; and the Royal Society did me the honour to deſire my thoughts on thoſe ſubjects. Accordingly, on the 25th of *November*, 1754, I ſent them a letter, to ſhew the cauſe of the aſcent of vapours and exhalations;

tions; and alſo to ſhew the cauſe of winds, both regular and erratic; and to explain the general phænomena of the weather, barometer, &c. which letter was alſo honoured with their acceptance, and printed in the Philoſ. Tranſ. for the year 1755. In this letter I mentioned ſome experiments, which the Society alſo deſired me to ſend to them; and they are alſo printed in the Philoſ. Tranſ. with my letter. The great honour which the Royal Society had hitherto done me, drew my attention cloſer to theſe matters, and made me think it neceſſary for me to enquire farther into the nature and properties of electricity; and, if poſſible, to diſcover the general laws of its action, before I proceeded on the other ſubjects which I had mentioned; that I may be enabled to write more intelligibly to the Society, and to myſelf, than I found it was poſſible to do from what was then known about electricity.

At this time I perceived that all electrified bodies exerted two different powers, an attractive and a repulſive power; and, accordingly, have ſaid in my letter, that when the particles of vapour were forced within their repulſive powers, they then ran into contact and ſpericity by their attractive powers; but, as yet, I knew nothing of theſe powers, or how they acted upon each other, or on the light parts of matter, by which they render their actions ſenſible to us. I therefore ſet myſelf to work upon electrical experiments, and, after having made a very great number, I found that electricity conſiſted of two different diſtinct ſubtile elaſtic mediums, which were equally and ſtrongly attracted to all matter, and that in proportion to the quantity of matter; and that theſe mediums ſtrongly attracted and greatly condenſed each other; ſo that when they were equal in quantity to each other, they were condenſed into an exceeding minute compaſs; but when one was leſſened, and the other increaſed in any body, the increaſed medium or power would, by its own elaſtic force, diffuſe itſelf through a ſpace many millions of times greater than that which it occupied in its condenſed ſtate; and yet may be inſtantly retracted to the body from which it iſſued, by ſuperinducing on that body a proper quantity of the other power; that theſe electric atmoſpheres repelled any atmoſphere ariſing from the ſame power, but attracted any atmoſphere ariſing from the different medium or power: ſo that all the known phænomena, ariſing from electricity, are cauſed by theſe mediums or powers being equally attracted by all matter, their ſtrong and equal attraction and condenſation of each other, and their great elaſtic force; for a more particular explanation of which, I muſt beg leave to refer your lordſhip to the letters which I am going to mention.

When I had gone thus far in my enquiry, I wrote a letter to the Royal Society on the 9th of *Auguſt*, 1757, to ſhew the exiſtence of theſe different powers in electricity, and to explain their manner of acting; and alſo to ſhew that magnetiſm conſiſts of like powers, which act by the ſame laws, tho' I do not by any means think them the ſame thing. As I did not hear what reception this letter met with, I thought I had not explained my meaning ſufficiently to be underſtood; and therefore, on the 14th of *February*, 1758, I wrote another letter, to explain this matter more fully by experiments, and to give general laws by which the electric powers act. I was ſomething ſurprized that I never heard any thing of theſe letters, becauſe I am as well aſſured of the truth of them, from numberleſs experiments, that two different diſtinct elaſtic mediums do exiſt in electricity, as I am that I live, or move, or have any being; and as I knew the ſubject to be of the greateſt importance, as the electric powers are the great principle of motion in the world; and that their actions can never be explained, without allowing theſe two different mediums. However, on the 26th of *March*, 1760, I wrote to the Royal Society, endeavouring to ſhew that the electric powers are the cauſe of the reflection and refraction of light; and alſo to Doctor *Birch*, with ſome electrical experiments, which I think cannot be explained by any thing that has been ſaid of electricity, without allowing the doctrine of two mediums which I have given; nor was it poſſible to conceive what I ſaid about the reflection and refraction of light, without a perfect conception of the action of the different powers of electricity. Theſe letters alſo paſſed unnoticed; I ſuppoſe for the reaſon I have now mentioned.

About the beginning of the year 1761, I received the vol. of Philoſ. Tranſ. for the year 1757. This book came thus late to my hands, by being obliged to ſend to *London* for it; for our bookſellers in this kingdom could not furniſh me with it, tho' I often applied for it.

In this volume I met with three letters, ſigned *Eraſmus Darwin*, Phyſician at *Litchfield*, *Staffordſhire*; which he is pleaſed to call a confutation of what I wrote about the cauſe of the aſcent of vapours and exhalations, &c. His firſt letter is addreſſed to Mr. *William Watſon*, with whom, he ſays, he is not perſonally acquainted, deſiring Mr. *Watſon* to lay his papers before the Royal Society, if Mr. *Watſon* thought that they contained truth; but if he ſhould deem them trifling, or futile, to ſuppreſs them.

His ſecond letter, addreſſed to the Royal Society, contains a theory of his own to explain the aſcent of vapours, which, he ſays, is founded on principles better known than mine; but, for my part, I cannot ſee any principles which he makes uſe of, but what contradict the reaſon and experience of all mankind (if ſuch can be called principles) except that, he ſays, that heat makes vapours aſcend; which, I think, every old woman knew before. His third letter, he ſays, is deſigned

ſigned entirely to deſtroy my theory, and nearly to demonſtrate, that the electric æther is far from having any ſhare in cauſing vapour to aſcend: how well he has done this his letter will ſhew. I ſhall only obſerve that his chief principle is, that the air is not acted upon by electricity. Now this is a flat contradiction of a truth that Mr. *Watſon* himſelf has, with a great deal of pains and labour, eſtabliſhed, and, I think, fully proved, by his experiments in *vacuo*, made for that very purpoſe; to ſhew that electricity is confined to non-electric bodies by the air. Philoſoph. Tranſ. vol. 47, page 362, &c. And tho' I know many experiments which prove the ſame thing, I chooſe to let Mr. *Watſon* ſee that he has recommended a doctrine entirely oppoſite to his own principles, and ſhall leave him to explain the reaſon.

Mr. *Darwin* has attempted to ſhew the fallacy of ſome experiments which I ſent the Society; but he has only proved the truth of them, and ſubſtituted a monſtrous fallacy of his own, and ſhewn that he is thoroughly ignorant in making electrical experiments; but as I have given ſuch an anſwer to Mr. *Darwin*'s letter, as I thought it deſerved, in my letter to the Royal Society, dated the 20th of *March*, 1761, I ſhall beg leave to refer your lordſhip to that letter; which was chiefly deſigned to give a farther explanation of the cauſe of thunder, and the aſcent and deſcent of vapours, from the diſcovery which I made that electricity conſiſts of two different mediums or powers, and their manner of acting on each other; which could not poſſibly be explained without this doctrine.

For when I wrote my letter to explain the cauſe of the aſcent of vapour, &c. I was conſcious of one defect in it; that is, I found it impoſſible, from what was then known of electricity, to ſhew how the whole quantity of electric fire, which aſcended and buoyed up the vapour, was ever to return to the earth again; and I knew that without ſuch a circulation, that this great operation of nature could never have been carried on in the ſame conſtant manner, as it has been done from the beginning of the world: but finding the reſt of my ſyſtem very conformable to the phænomena of nature, and hoping that a farther reſearch into the nature and properties of electricity may ſome way or other explain this matter, I let my letter go with that known defect. But ſince I have diſcovered the great elaſtic force of the electric mediums, by which either of them can (when freed from the attraction and condenſing force of the different power) diffuſe itſelf through a ſpace of more than a million of millions of times greater than what it occupied in its condenſed ſtate, and yet be retracted and condenſed to its former ſtate, by an addition of the different medium or power; it is eaſy to ſhew how electrified vapour may be carried up by the expanſion of the electric medium, and remain there till it meets with other vapours electrified with the different medium of electricity; in which caſe they muſt attract each other, and run into contact to form larger drops; and the electric mediums being then rendered nearly equal to each other in theſe drops, by an exchange of the different powers with each other in each drop, their atmoſpheres muſt be condenſed into almoſt an inſenſible compaſs, and therefore cannot longer ſerve to buoy up the vapours, which muſt then fall in rain.

And thus the whole quantity of both the electric mediums which, in their expanded ſtate, buoyed up the vapours, may, in a condenſed ſtate, return with them to the earth.

Now, that this is the real ſtate of the clouds; that is, that clouds in near approach to each other are frequently electrified with the different mediums or powers of electricity, I have proved by a great number of experiments on the clouds; and have always found, that when two ſuch clouds have drawn each other into contact, ſo as to let fall a heavy ſhower from the middle, that one end of ſuch cloud was electrified with one power, and the other end of the cloud was electrified with the different power of electricity, while the middle of the cloud or ſhower ſhewed little or no ſign of being electrified. Mr. *Canton* of the Society has alſo often found the clouds electrified with the different powers, which he calls poſitively and negatively electrified. This, my lord, would be ſufficient to prove the truth of what I have laid before the Royal Society; but ſince my writing the laſt mentioned letter, I have gone much farther to evince the truth of it; for I have fitted up an apparatus, by which I can make an artificial ſhower of rain with the electrical powers. For by electrifying two fountains of water with the different powers, they ſhall be diſſipated into very ſmall particles in their aſcent, which particles ſhall attract each other above, and run into larger drops, and come down in the exact form of rain from the clouds. This, I think, comes as near to a demonſtration as the nature of the proofs will admit. The only difficulty remaining is to ſhew how different clouds come to be electrified with the different powers; but the want of this knowledge does not any way detract from the truth of what I have advanced, ſince the clouds are really found to be ſo electrified.

But, my lord, this doctrine of electricity is not only uſeful to explain this important phænomenon of nature, but very many others which have puzzled the philoſophers from the firſt ages; and modern philoſophers cannot be ignorant, with what ardent zeal ſuch active principles of attraction and repulſion were ſought for, but left unknown, by the great *Newton*'s ſelf; yet not ſo far unknown, but that, in one great gueſs, he has expreſsly named the thing; tho' at that time he had not any poſſible means of proving the truth of his conjecture. For in his 31ſt Query, he ſays, 'The attractions of gravity, magnetiſm, and 'electricity, reach to very ſenſible diſtances, 'and ſo have been obſerved by vulgar eyes; 'and there may be others which reach to ſo 'ſmall diſtances as hitherto to eſcape obſervation; and perhaps electrical attraction 'may reach to ſuch ſmall diſtances, even without being excited by friction.' Which I now think it certainly does; and as I find the electric mediums or powers univerſally adheſive to all matter, and that they can both attract and repel from very minute to very great diſtances, they are fitted to anſwer many more of his purpoſes than could be then imagined, as they were ſuppoſed to reſide but in a few particular bodies, and their manner of acting was quite unknown. I beg leave to add a few more lines from the latter end of this query, where he ſays, 'To tell us that every 'ſpecies of things is endowed with an occult 'ſpecific quality, by which it acts and produces manifeſt effects, is to tell us nothing: 'but to derive two or three general principles 'of motion from phænomena, and afterwards 'to tell us how the properties and actions of 'all corporeal things follow from thoſe manifeſt principles, would be a very great ſtep 'in philoſophy, tho' the cauſes of thoſe principles were not yet diſcovered; and, therefore, I ſcruple not to propoſe the principles 'of motion above-mentioned, they being of

‘ very general extent, and leave their cauſes ‘ to be found out.’

This whole query, which conſiſts of many pages, was deſigned by that great man to ſhew the neceſſity of the exiſtence of principles of motion, many of which may not as yet be diſcovered, and to recommend a farther reſearch into the nature and properties and laws of action of theſe we do know; three of which he has particularly mentioned; viz. gravity, magnetiſm, and electricity. Gravity, tho’ it may be the cauſe of compound motions in bodies put in motion by ſome other powers, I think is rather a principle of reſt, as it inclines all bodies to move to each other in right lines, with a force proportioned to their quantity of matter and their diſtance; which motion once finiſhed, gravity cannot again put them in motion. The other two ſeem to me to be more properly principles of motion, as they can both attract and repel, and are therefore fitted to preſerve the motions neceſſary to vegetation, animal life, &c. Magnetiſm, as it is found in no other matter but iron, cannot be of very general uſe; however, it may be of much more uſe than we can at preſent know of. Electricity, as it is univerſally adheſive to all matter, and of great power, muſt be of very general uſe in the operations of nature; many of which I can point out.

Now the papers which I have laid before the Royal Society, and which have paſſed

unnoticed,

unnoticed, go a great way in explaining the nature and properties of the two laſt mentioned principles of motion, and in giving general laws, which will explain all the known phænomena which ariſe from magnetiſm and electricity. I would therefore beg your lordſhip, that theſe papers may be carefully reexamined, and printed in the Philoſophical Tranſactions; for as they are true, they muſt one day be found to be ſo, and will ſave others ſome part of the labour and pains which it has coſt me for ſome years paſt to come at thoſe truths.

But if any gentlemen of the Society think thoſe papers unintelligible, or deficient in explaining the truths which they contain, I ſhall be greatly obliged to them if they will furniſh me with their objections or obſervations, by letter, and I will return the moſt ſatisfactory anſwer in my power; and I believe I may ſet the matter in a better light now, than when I wrote; for I have had great experience in electricity ſince I wrote thoſe letters.

I know I have mentioned many experiments, in my papers, which may appear very improbable, if not impoſſible, to many gentlemen who have made electrical experiments; but I ſolemnly aſſure you, that I have not mentioned one which I have not repeated an hundred times; nor did I mention them with any other view than that it may be known, when others come to make the like experiments, that I was not ignorant of ſuch matters when I wrote thoſe papers.

But, my lord, I have ſince performed much more ſurprizing experiments on the human body, merely by the electric powers, in curing many paralytic and rheumatic caſes, the cure of agues, chloroſis, and other diſorders. And ſome of thoſe cures were of ſo extraordinary a nature, and out of the power of the known *materia medica*, that our phyſicians here looked upon them as little leſs than miraculous; and in all theſe cures I have been enabled, from what I know of electricity, to proceed from a reaſoning *a priori*, which I think nobody elſe has done in the application of electricity to medicinal uſe; at leaſt I have not met with any account of it. For all the attempts to cure diſorders by electricity, which are mentioned in the Philoſ. Tranſac. appear to me to be made without any previous rational deſign; and accordingly, they have done harm as well as good; and I believe that the accounts of miſchief done would be more numerous, if all were as honeſt to confeſs it, as the gentlemen who have told of their diſappointments; for I know that great miſchief may be done by a wrong application of electricity in a medicinal way, and therefore gave a hint to the unſkilful, not to attempt the uſe of it, in my laſt paper to the Society.

But I alſo know, that when it is rationally applied, that it may be the moſt powerful medicine, in many caſes, that has ever been diſcovered; which I have ſufficiently experienced for ſome years paſt.

I offered to give ſome accounts of this matter to the Royal Society; but as I did not hear from the Society, I ſuppoſed that they were not thought acceptable.

Though I fear that I have already tired your lordſhip's patience, I cannot forbear expreſſing my ſurprize at the unwillingneſs of ſome gentlemen to admit that electricity conſiſts of two different diſtinct elaſtic mediums, ſince it may be demonſtrated by almoſt every electrical experiment; and that there is not one ſingle phænomenon, ariſing from electrical experiments, which can be fully explained without it. And it is the more extraordinary, as I find it has been thought all along that there were two kinds of electricity, but that they ſubſiſted in different matters, as glaſs and reſins, &c. and this as early as Mr. *Du Fay*; and Mr. *Muſchenbroek* has expreſsly ſaid, that there were two kinds, and that any body may be endued with either kind, but that nobody could be in poſſeſſion of both together: in which this gentleman was ſo far miſtaken, that it is impoſſible to ſeparate them entirely in any body. Mr. *Benjamin Franklin* gives another doctrine, which has been followed, and ſays, that glaſs throws out the electric fire, and that reſins drink it in; that is, that glaſs can ſuperadd to any body a greater quantity of electric fire than its natural ſhare, and that reſins can take part of its natural ſhare from any body; which he calls poſitive and negative electrifying; and ſuppoſes that bodies repel each other the more, the more they are diveſted of their natural ſhare, in the ſame manner they do when there is an addition to their natural ſhare. With deference to that ingenious gentleman, I can't forbear thinking it an odd way of reaſoning, to think that bodies ſhould repel each other the more, the more they are diveſted of the repelling power: but this is all a miſtake; for glaſs neither throws it out, nor does wax drink it in; for in their natural ſtate they are both endued with an equal ſhare of both the powers; and theſe powers are brought into ſenſible action, by encreaſing one power and leſſening the other; and then the encreaſed power expands itſelf into an extenſive atmoſphere, which we call electricity; and either power may be encreaſed on the wax or on the glaſs; that is, I can make the glaſs cylinder act as wax commonly does; and when the wax cylinder acts as glaſs, and almoſt as ſtrong as glaſs; in the ſixth part of a minute I can make it act as wax again, and this with the ſame rubber; or, with a large piece of ſealing-wax, with the ſame rubbing, I can electrify a piece of bog-down, ſuſpended by ſilk, with either of the powers, and then withdraw that power with the ſame wax, and electrify it with the contrary power; and thus as often as I pleaſe;

and a ſpectator ſhall not perceive any difference in my manner of touching the down with the wax, to give either one power or the other, as he pleaſes to direct: now this is a full demonſtration that both powers do exiſt without ſide the wax; or how is it poſſible for me to electrify with which ever power a ſpectator pleaſes to chooſe? Theſe, and a great number of other experiments, which demonſtrate, to the meaneſt capacity, the exiſtence of theſe different powers in all bodies, I have ſhewn to hundreds of gentlemen, who can atteſt the truth of what I write, if required. And in my letter of the 14th of *February*, 1758, I have given experiments, which, if carefully examined, will ſufficiently prove the doctrine of electricity which I have therein given: but if any gentleman is of the contrary opinion, I will ſubmit to this trial: that if he can point out any one known phænomenon ariſing from electrical experiments, which I cannot fully explain by this doctrine; or if he can produce any one ſingle phænomenon, which he can fully explain, without admitting of two powers in electricity; I will ſubmit and confeſs my ignorance.

But I muſt beg that this diſquiſition may be carried on by letter; and theſe letters ſhall be faithfully laid before the Royal Society, if the gentleman pleaſe, that they may judge on whoſe ſide truth reſts. I chooſe this epiſtolary correſpondence, becauſe my life is too far advanced to wait for anſwers publiſhed in the Philoſ. Tranſ. which muſt come late, or perchance never reach my hands.

I believe that it is unneceſſary to ſay any thing more on the importance of this enquiry, to aſcertain general laws by which the actions of theſe general principles of motion may be explained; and therefore I ſhall conclude, in requeſting your lordſhip, that I may have the honour of one line, to let me know that this letter gets to your hands. I am,

Your Lordship's,

Moſt humble and

moſt obedient ſervant,

Henry Eeles.

To the Right Honourable
George, earl of *Macclesfield*, Preſident of
the Royal Society.

⁂ My Printer informed me, during the printing of the foregoing work, that there would be a few pages blank at the cloſe of thoſe letters; I am therefore willing to fill them with an experiment or two, for the farther illuſtration of the reciprocal exchange of the electric powers.

FIX a wire to the under part of a coated bottle, ſo that the point may ſtand upright; and on that point place the needle with the reverſed points. Place this bottle on an electric ſtand, with a communication from the conductor. All the time the bottle is charging, the needle will turn; but when the bottle is charged, the needle ſtops. Then touch the top of the bottle with your finger, or any conductor, and the needle will turn till the bottle is diſcharged. Now while the bottle is charging, if you touch the needle with a piece of bog-down ſuſpended by ſilk, you'll find it electrified by the vitreous power, which flies off in exchange for the reſinous power drawn in from the air to the outſide of the bottle; and while the bottle is diſcharging, if you touch the down in the ſame manner to the needle, you'll find it electrified with the reſinous power, which flies off from the outſide of the bottle in exchange for the vitreous power drawn in through the points from the air; while the vitreous power from the inſide of the bottle makes the ſame exchange for the reſinous power through your finger, to make theſe different powers equal to each other, withinſide and withoutſide the bottle.

Place two *Leyden* bottles on an electric ſtand, with their coatings in contact; and while you charge one from the conductor, let a perſon on the floor touch the top of the other bottle with his finger; you'll find the firſt bottle charged with the vitreous power inſide, and the ſecond with the reſinous power inſide. Now the exchange here is evident; for while the reſinous power from the inſide of the firſt bottle changes place with the vitreous thrown in from the conductor, the vitreous, from the coating, changes place for ſo much of the reſinous from the coating of the ſecond bottle; and the vitreous in that bottle changes place for ſo much of the reſinous power drawn in through the man on the floor.

I could eaſily furniſh experiments ſufficient to fill a great deal of paper; but I ſhall only mention one or two which I often made, and have ſhewn to many gentlemen verſed in this ſcience, which perhaps may puzzle the writers on this ſubject: but whoever can make them, muſt and will underſtand my doctrine of electricity; and plainly diſcover, that all that has been ſaid about poſitive and negative electricity, or a plus and minus of the ſame power, has been to little purpoſe, and only ſerved to keep men in the dark about this ſcience.

I have a glaſs globe, intirely ſmooth, which, with the ſame rubber, I can make throw out the reſinous or vitreous power, and electrify other matters with each power as often as I pleaſe; and generally I can charge theſe powers in one revolution of the globe; but in two or three revolutions I never fail to do it.

I have another glaſs globe intirely ſmooth, ſupported by one glaſs neck, from the equatorial line of which globe I can electrify with the reſinous or vitreous power as I think proper, without applying a hand or any rubber to the globe, or any friction from the things to be electrified; and what is more, I can electrify two things with the different powers at the ſame time.

I hope my readers will forgive me, for not explaining theſe experiments, till I ſee who can make them, and explain them by the doctrine of poſitive and negative electricity, or a plus and minus of the ſame power.

As I have ſeen Mr. *Prieſtley* quoted, for improving and methodizing the theories of Mr. *Dufay*, and Mr. *Symmer*; I muſt obſerve, that Mr. *Dufay* never thought of the co-exiſtence of theſe different powers in all bodies; nor even Mr. *Muſchenbroeck*, long after him; therefore, there was not any theory of Mr. *Dufay*'s to be methodized. Theſe papers will appear to have been wrote to the Royal Society, before Mr. *Symmer* thought any thing of the matter; however, as far as he has gone, he was right; and I think that Mr. *Prieſtley* has made too much of a trifling objection to what Mr. *Symmer* has ſaid, that any man, from his own ſenſations, would be convinced that the powers came in oppoſition from both ſides of the bottle, by feeling a ſlight ſhock in his wriſts, a ſtronger in his elbows, and ſo on. Mr. *Symmer* did not ſay that theſe powers did not circulate through the body; but, that this ſenſation would ſhew their different direction: And, an explanation of theſe different ſenſations depends on a knowledge of anatomy, and the action of theſe powers on the different parts, which, it is poſſible, that both Mr. *Prieſtley* and Mr. *Symmer* may be ſtrangers to. But this objection was only a pretext to mend Mr. *Symmer*'s diſcoveries. Now, I believe it will plainly appear to any gentleman, who does me the honour to read theſe papers with attention, that the only theory which Mr. *Prieſtley* could have methodized, muſt be what he took from theſe papers; if an hypotheſis can be ſaid to methodize a doctrine which was clearly proved by experiments, ten years before: But I would aſk which of Mr. *Prieſtley*'s own numerous experiments, or what experiments of any other man he has explained by his hypotheſis?

or how he has ſupported this hypotheſis, or whence he took it? For, he has not produced a ſingle experiment in aid of it, to ſhew the exiſtence, or manner of acting of theſe different powers: Or, I would aſk whether his hypotheſis has added any thing to the truth, or explanation of what I have wrote? or, if I had happened to have wrote that hypotheſis, whether I ſhould have put this matter in a clearer light than what I have done? or, what man would have depended on that hypotheſis, without any other proof? So that I think Mr. *Prieſtley* may fairly own from whence he framed his hypotheſis; for, after reading theſe papers, I believe that his hypotheſis will not be found of any great uſe. Mr. *Prieſtley* has artfully ſhut me out of his hiſtory of electricity; though all theſe papers were addreſſed to the Royal Society, of which he is a member; and it appears from the firſt of them, that my attempt to ſhew the electric powers were the cauſe of thunder, was approved by that Society; and is the only attempt, of that kind, which ſtands recorded in the Philoſophical Tranſactions. As for the reſt of them, with Mr. *Prieſtley* and his hypotheſis, I muſt leave to the judgment of unprejudiced readers.

FINIS.

ERRATA.

In the PREFACE.

Page xxxiii, line laſt, *for* its electricity, *read* its elaſticity.
xxxvii, l. 16, *for* Mr. *Gregg's*, *read* Mr. *Gray's* time.
xlvii, l. 21, *inſtead of* for, *read* from the then known powers of nature.

In the LETTERS.

Page 19, l. 16, *for* united, *read* emitted.
23, l 28, *for* incenting, *read* meeting.
37, l. 20, *for* united, *read* emitted.
90, l. 13, *for* miles, *read* turns.
93, l. 25, *for* power to, *read* power on.
112, l. laſt but two, *for* officers, *read* offices.
131, l. laſt but one, *for* experiments, *read* experimenters.
165, l. 24, *for caminæ*, *read laminæ*.